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THE PSYCHOLOGICAL REVIEW

THE LAW OF EFFECT

A ROUND TABLE DISCUSSION ¹

HARVEY A. CARR

University of Chicago

I

In extending the invitation to give this introductory paper, your chairman offered one suggestion that I formulate my views on several of the controversial topics in the field of learning, and then be prepared to defend these views against the onslaughts of this audience. It is admitted that such a contest of trenchant criticism and witty repartee would be highly exciting and entertaining—to the audience—but I doubt its value from the standpoint of science. Such discussions often tend to emphasize and fixate our divergences of belief and opinion without contributing an iota to our mutual understanding.

I once heard it said that the object of science is the search for truth, and that some degree of social agreement is one of the characteristics of truth. If these propositions are true, it would seem that scientists should be striving for mutual understanding and agreement. Naturally differences can and do arise, but presumably these are of value only in so far as

¹ Editor's note: The Round Table took place in Minneapolis on September 2, 1937, at the annual meeting of the American Psychological Association. It was arranged and conducted by Professor Hulsey Cason of the University of Wisconsin. Professor Carr was asked to choose a phase of learning for discussion and to present his point of view in a twenty-minute paper, to be followed by five-minute discussion by each of the other five members of the Round Table. Several of the members, including Professor Carr, had to leave before the general discussion. These discussions from the floor consisted of questions and comments which Professor Thorndike answered. They revolved entirely around Professor Thorndike's contribution and served to clarify his position.

they aid in the search for further truth. However, we should never let these differences blind us to the unanimities already attained, nor prevent us from attempting to compose our differences and attain further agreement. It is agreement, and not disagreement, that is the object of science. Disagreements arise easily and naturally, and I have often suspected that we are all somewhat averse to agreeing with some one else. Agreement is something that we must strive to attain, and I have often been impressed with the amount attained by two people holding diverse views who seriously try to understand each other and seek common points of agreement.

As a basis for this discussion, I shall propose a series of questions and propositions concerning the Law of Effect which are designed to discover the degree of unanimity already existing, the point at which our differences emerge, and the basis for these divergences. I will admit that this is not a very exciting or entertaining program, but it is one that is more consonant with the spirit of science.

In lecturing I have frequently referred to the alleged influence of food and electric shock in determining the final outcome of the selective and eliminative process, and some students have made rather pointed remarks that the Law of Effect has been thoroughly discredited and discarded. A colleague once remarked that the Law of Effect is a dead issue. I raised the question whether this statement meant that the attainment of food and the application of shock did not influence the nature of the act that was finally established. He replied that so far as he knew practically everyone admitted this influence, and that he was using the term Law of Effect to refer to Thorndike's explanation of this fact in terms of pleasantness and unpleasantness. These two incidents will serve as an introduction to the following series of questions.

1. Does there exist, as alleged, a high degree of unanimity of opinion that the food and the shock do determine to a large extent the final product of the selective and eliminative process?

2. Is it not legitimate and proper to refer to this alleged fact as some kind of a Law of Effect? If so, let us refer to it for the purpose of this discussion as the empirical law of effect.

3. Granted this fact, are not Watson's proposed Laws of Frequency and Recency to be regarded as empirical explanations, in that he attempts to explain this fact in terms of some other *observable* data?

In the absence of an established empirical explanation, we are forced to resort to the hypothetical and theoretical type of explanation, namely, that the influence of food and shock is to be explained in terms of their pleasant and unpleasant, their satisfying and dissatisfying, their beneficial and detrimental effects, etcetera.

4. Do not our divergences arise at the point of explanation and especially when we reach the level of the theoretical type of explanation?

5. Do we not tacitly assume that there is but one law of effect? What is this law of effect? Does it refer to the influence of food and shock on learning, or does it refer to the explanation of this fact in terms of pleasantness and unpleasantness? If it refers to this affective mode of explanation, why may there not be as many laws of effect as there are proposed modes of explanation? May not the meaning of the term vary with the individual, and hence this ambiguity of the term be the source of much of our confusion and misunderstanding?

6. Is it proper to call a theoretical explanation a law? Does not a law at least denote an established and accepted fact, and in this case should not the term be used to refer only to what I have called the empirical law of effect? To what extent have we been guilty of calling theoretical explanations laws in order to induce our readers to accept them as factual in character?

7. What does Thorndike mean by the law of effect? Does he, as is so often stated, use the term to denote the selective effects of pleasantness and unpleasantness, or the differential effects of pain and food irrespective of the theoretical nature of the intervening mechanism?

I have long suspected that Thorndike uses the term to refer to the influence of pain and food, and I have sometimes suspected that he uses such terms as pleasantness and unpleasantness, satisfying and dissatisfying, beneficial and detrimental, more as descriptive than as explanatory concepts. I have been led to this belief by some of his recent experiments and by a number of his conversational remarks. It is my impression that his guessing experiment was designed primarily to prove that the experimenter's responses of 'yes' and 'no' did influence the nature of the subject's subsequent guesses, and in my opinion this simple experiment did amply demonstrate this fact, and, as I recall, Thorndike made no attempt to explain this fact in terms of pleasantness and unpleasantness.

I make no pretense of knowing the nature of the mechanisms by which the attainment of food, the reception of the shock, and the responses of 'yes' and 'no,' etc. do affect the subsequent course of learning. Many theoretical possibilities have been suggested.

8. Have we not been too much dominated by the desire for explanatory simplicity? Have we not tacitly assumed that only one of these suggested mechanisms can be true, and hence that the various alternatives proposed are mutually exclusive?

9. Is not the hypothesis of multiple mechanisms the more logical and serviceable one? Why should not the response of 'yes' operate in a different manner than the attainment of food? Given the same factor shock, why may not the mechanism of connection vary with the organism such as a rat or a human being? Given the same organism, why may not the way shock operates vary with the behavior situation in which it occurs?

In reference to the last question I may refer to the results of Muenzinger's experiment. Previous to this experiment, it was generally assumed that shock influences the subsequent course of learning by inhibiting that component of the act that leads to pain. Obviously this explanation will not work for his shock-right condition. There are at least two possi-

bilities: (1) We may attempt to formulate a single mechanism that will account for both results, heightened attention, for example; or, (2) we may assume that the mechanism differs for the two conditions.

10. Is not the hypothesis of multiple and variable mechanisms at least a logical one, for will not a complex organism like man offer many possibilities for connection between a stimulus and its subsequent behavior on the next day?

I now wish to raise some questions concerning the nature, the kinds, and levels of explanation. By the explanation of a fact I arbitrarily refer to the establishment of a contingent relation of that fact to *something else*. Explanations can then be classified on the basis of the nature of this something else. This may be an observable phenomenon or a conceptual object, and hence we have the distinction between the empirical and theoretical types of explanation. This other thing may be a psychological, a physiological, a physical, a theological, or a metaphysical concept, and hence our mode of explanation may be classified as psychological, physiological, physical, theological, or metaphysical in nature. The concept of levels of explanation needs to be briefly illustrated.

In the process of learning, we observe that certain components of the initial act are preserved or selected, while all other components gradually disappear or are eliminated. This selection of components X and Y and the elimination of the other components is an *observational fact*, and it constitutes the *primary empirical datum* which we wish to explain.

Suppose that we explain this fact in terms of the influence of such factors as food, shock, yes, and no. This is an empirical, psychological, and first level type of explanation.

If the above explanation is accepted, we are then confronted with the task of explaining the relation of food and the fact of selection in terms of some intervening mechanism. Let us suppose that we do this in terms of pleasantness and unpleasantness. We have thus explained our first explanation, and our second explanatory attempt is theoretical, psychological, and second level in character.

The next step is the attempt to explain the connection

between pleasantness and unpleasantness on the one hand and selection and elimination on the other in terms of some intervening mechanism. This would be a third level type of explanation. Presumably it would be theoretical in nature, and it might be couched in either psychological or physiological terms.

In this process of seeking deeper and deeper levels of explanation, one might pass through the field of psychology and enter that of physiology, and then go on to the fields of biochemistry and physics. Again one might skip the field of psychology, and begin seeking for physiological explanations of an empirical or theoretical nature. On the other hand one might eschew all physiological explanations, and attempt to explain the phenomenon directly in terms of such physical concepts as the behavior of electrons and protons, or fields of mechanical forces. However, others may disdain all such mundane intermediaries, and proceed at once to such concepts as spiritual forces or Aristotelian forms.

This diversity of possible explanatory procedures at once raises the question of their relative worth, and indeed one often encounters from time to time certain statements that seem to imply a definite tacit assumption concerning relative values.

We encounter many comments concerning the statement that the outcome of the selective and eliminative process is due in part to the influence of food and shock. Some deny that it is an explanation, and some assert that it is a poor explanation in that it is but partial and incomplete in nature. The comments seem to imply that only final and complete explanations have any value.

Some deny the existence of psychological explanations, while others deny their value and assert that only physiological or physical explanations have any reality or value.

It is almost a universal custom to carry our explanatory analysis at least to the first theoretical level, and there is much to justify such a procedure. Customs seem to become easily transformed into cardinal virtues, and we encounter some disposition to make the tacit assumption that only the

theoretical, as opposed to the empirical, type of explanation has any value or reality.

This question of the relative values of diverse explanatory procedures has always intrigued my reflective curiosity, and I am proposing a series of questions in the hope of securing some illumination.

11. In the present state of knowledge, is there such a thing as a final and complete explanation of any phenomenon? Is not the search for final and complete explanations just as futile a task as the search for the pot of gold at the end of the rainbow?

Is not the place where we decide to stop regarded as the final explanation, and is not this place determined primarily by our particular interests? Is not the alleged finality and completeness of a physiological type of explanation merely relative to the field of our psychological interests?

If we believe that final and complete explanations are absolute in character in the sense that it is impossible to do anything more about them, do not theological and metaphysical types of explanation best meet this criterion?

12. In comparing the value of any two procedures, must we not recognize the fact that their relative worth is a function of the standard of evaluation employed? Are there not various legitimate standards of evaluation, hence may not the relative worth of two procedures vary with the standard of evaluation? May not one procedure be better than another for certain purposes, and yet be the poorer when regarded from another standard of reference?

Let me illustrate these questions in terms of a few accepted standards of evaluation.

13. Two proposed explanations may be evaluated from the standpoint of their empirical truth. Of two proposed explanations, is not the better one that whose truth can be tested by experimental and correlational means? From this standpoint, are not the empirical types of explanation better than most theoretical ones, and are not theological and metaphysical types of explanation the worst of the lot?

14. We may evaluate explanations from the standpoint

of practical psychology—the *control* of the phenomenon that is being explained. We indirectly control phenomena by directly manipulating their causal conditions. Let us assume that we have two explanations of the same phenomenon. Both are empirical in character, and both are accepted as true. They differ in respect to level of explanation, and one is psychological and the other physiological in character. From the standpoint of control, is not the psychological type of explanation more likely to be the better one?

15. Explanations may be evaluated from the standpoint of systematization—of relating the phenomenon in question to a wider set of data. Is not the theoretical and speculative type of explanation extremely serviceable for this purpose?

16. Explanations may be evaluated from the standpoint of their function in promoting research and the acquisition of further knowledge. Do not many types of theoretical and speculative explanation find their chief value in serving this function?

For the purposes of this questionnaire, let us assume that the truth of any comparative judgment of worth and importance is relative to some particular standard of evaluation, and that a judgment may be based upon any one of several legitimate standards. On the basis of this assumption, the following series of questions may be proposed.

17. Can any *general* statement of the superiority of a particular mode of explanation be true?

18. Are not some of our differences and misunderstandings due to the fact that we are employing different, although legitimate, standards of evaluation?

19. May not some of our judgments be erroneous because they are based upon illegitimate and unacceptable standards?

20. Would not our differences and misunderstandings tend to disappear if we were careful to specify the nature of the standard employed?

The possibility of using illegitimate and unacceptable standards of evaluation deserves some exemplification. It is an accepted truism that psychology, to be scientific, must be *objective*, but there seem to be two distinct interpretations of

the meaning of this term. The term may refer to the character of the data, or to the attitude of the scientist toward his data.

There are those who seem to accept the second interpretation, and to assume that it is entirely possible for a scientist to adopt a highly objective attitude even towards the so-called subjective type of data.

On the other hand, there is a conspicuous group of psychologists who seem to pride themselves on being the sole possessors of this *virtue of scientific objectivity* because they deal only with objectively observable data and attempt to couch their explanations of these data in terms of physical or physiological concepts of either an empirical or hypothetical character.

Perhaps we have here uncovered a tacit standard of evaluation that is used to support in part the alleged superiority of physical and physiological types of explanation, and on the basis of this assumption we may raise two further questions for consideration.

21. Do any of the supporters of the superiority of physical and physiological explanations base their judgments in part upon the tacit assumption that these modes of explanation are superior because they are consonant with the concept of scientific objectivity and hence are suffused with its virtue?

22. Is their usage of the term the correct and proper one, and if not, is not this assumed standard of evaluation illegitimate and unacceptable in character?

This completes the series of questions, and I only regret that it was not feasible to present them to this audience in the form of a questionnaire, and have the results assembled, tabulated, and distributed before proceeding with the discussion.

II

EDWARD CHACE TOLMAN

University of California

I would answer all of Professor Carr's questions in the affirmative. And yet I suspect there may be one point with regard to which he and I would still disagree. It is an empirical point—and this in spite of the fact that his first and essentially empirical question was so framed that I could hardly object to it.

His first question read:

"Does there exist, as alleged, a high degree of unanimity of opinion that the food and the shock do determine to a large extent the final product of the selective and eliminative process?"

My answer would be, of course, "yes." I agree and I should suppose that we all of us here would have to agree that *in the end* the animal does tend to go in the direction of food and away from the direction of shock (assuming, that is, the orthodox set-up where food is in one direction and shock in another).

But there are further empirical facts concerning the ways in which this final situation can be brought about which seem to me equally important for determining one's final formulation of the laws of learning or the theory of learning.

These further empirical facts, which I have in mind, are: (1) that of 'latent learning'; (2) that of the reverse phenomenon—the jump-up in errors which occurs when a reward has been suddenly removed or changed to one of lesser value; and (3) the fact that two alternative habits, one leading to food and the other to water, can be built up, such that on the days when the animals are hungry they will utilize primarily the getting-to-food habit and on the days on which they are thirsty they will utilize primarily the getting-to-water habit.

You will remember that Blodgett² discovered that, if hungry rats be run through a maze but without food at the end, their apparent or 'manifest' error curve drops, comparatively speaking, but little. If, however, after some ten or more such days with no reward (or only the little reward of being taken out and returned to the home cage) the rats on some fine bright morning be made to find food at the end of the maze, then on the subsequent day their numbers of errors will be found to be greatly reduced. It appears, in short, as if during the non-reward period they must have learned in some 'latent' form a great deal more than their behavior ever showed, so that (to speak anthromorphically) as soon as it was made 'really worth their while,' they could at once and without further practice exhibit and make manifest this latently acquired learning. As long as they received no food they continued to enter many blind alleys. None the less they were in some sense learning which *were* the blind alleys and which *were* the true path segments. For, as soon as they had once found food at the end of the maze, they could then *on the very next trial, without any further practice*, choose the true path segments practically 100 per cent of the time.

And you will remember that the reverse phenomenon has also been found by myself and Honzik³ and by Elliott⁴ and by Bruce.⁵ If, after rats have learned a maze under ordinary reward conditions, this reward be removed or a less desirable one substituted, then on the immediately subsequent trial they will wander around and go into practically all the blind alleys. But this hardly seems to mean that they have unlearned the maze as a result of that one non-reward experience. For, if the previous reward be introduced again,⁶ they

² H. C. Blodgett, The effect of the introduction of reward upon the maze performance of rats, *Univ. Calif. Publ. Psychol.*, 1929, 4, 113-134.

³ E. C. Tolman and C. H. Honzik, Introduction and removal of reward and maze performance in rats, *Univ. Calif. Publ. Psychol.*, 1930, 4, 257-275.

⁴ M. H. Elliott, The effect of change of reward on the maze performance of rats, *Univ. Calif. Publ. Psychol.*, 1928, 4, 19-30.

⁵ R. H. Bruce, The effect of removal of reward on the maze performance of rats, *Univ. Calif. Publ. Psychol.*, 1930, 4, 203-214.

⁶ M. H. Elliott, *op. cit.*

can again without further practice make practically a 100 per cent score.

Finally, you will recall that Hull ⁷ and more recently and more decisively Leeper ⁸ have both demonstrated that rats can be taught to take one path on the days that they are hungry and another path on the days that they are thirsty. This shift in path appears each day, furthermore, on the first trials, that is to say, as soon as the new motivation conditions can express themselves. According to Hull this is merely evidence that among the stimuli to which the two sets of specific skeletal responses get conditioned are the organic stimuli coming from the hunger and thirst conditions, respectively. But according to Leeper's interpretation (and my own) this would mean also that the learning is not to be conceived as the setting up of specific S-R connections but as the acquisition of two independent sets of 'information' each of which can be drawn upon by an appropriate motivation.

In short, it seems to me as Lashley ⁹ first suggested and as Elliott ¹⁰ and especially Leeper ¹¹ have further emphasized that we must make a distinction between 'learning' and 'performance' or (to use Leeper's terms) between 'acquisition' and 'utilization.' Under the usual motivation and reward conditions performance or utilization may be conceived to mirror learning or acquisition in pretty direct fashion. But without reward or with reduced reward the learning or acquisition may be much faster than appears in performance or utilization.

⁷ C. L. Hull, Differential habituation to internal stimuli in the albino rat, *J. Comp. Psychol.*, 1933, 16, 255-273.

⁸ R. Leeper, The rôle of motivation in learning: A study of the phenomenon of differential motivational control of the utilization of habits, *J. Genet. Psychol.*, 1935, 46, 3-40.

⁹ K. S. Lashley, Learning: I. Nervous-mechanisms of learning, in 'The Foundations of Experimental Psychology,' Worcester, Mass.: Clark University Press, 1929, pp. 524-563.

¹⁰ M. H. Elliott, Some determining factors in maze performance, *Amer. J. Psychol.*, 1930, 42, 315-317.

¹¹ R. Leeper, The rôle of motivation in learning: A study of the phenomenon of differential motivational control of the utilization of habits, *J. Genet. Psychol.*, 1935, 46, 3-50.

But, if you can be made to agree to all this, then it seems to me that you must agree that the formulation of the Law of Effect has to be reconsidered. We can still accept the empirical statement that rats (and of course humans as well) tend in the end to go towards food and away from shock. But this now becomes a statement not about learning *per se* but about learning plus utilization or performance. And this means, further, that learning can no longer be envisaged as a matter of the building up of S-R connections but that it must be considered rather as learning of "what leads to what." When the further 'whats' resulting from the going in given directions are made good, given the animal's motivation, then, if the animal has also learned, he will tend to go in those directions. When the further 'whats' are made bad, given this motivation, then, in so far as he has also learned, he will tend not to go in such directions. Learning *per se* is to be envisaged, in short, as a matter of the animal's acquiring the correct 'expectation-sets'—the correct 'hypotheses'¹² or, to go back to the 17th, 18th and 19th centuries, the correct associations of ideas (not the correct S-R's).

This, as you see, lands me in the proposal of something to be called a 'psychological' explanation. But Professor Carr would also accept psychological explanations as perfectly good and respectable.

I wish, however, as one last word to stress that my psychological concepts of 'expectation-sets' or 'hypotheses' or 'sign-gestalten' (to use my earlier term) are, as I conceive them, always functionally definable affairs, not introspectively got at conscious contents. I doubt if rats, or even human beings, for that matter, have introspectively get-at-able conscious contents.

¹² This is Krechevsky's term. See I. Krechevsky, 'Hypotheses' in rats, *PSYCHOL. REV.*, 1932, 39, 516-532.

III

E. L. THORNDIKE¹³*Columbia University*

I, too, agree with the reasonable and balanced statement by Professor Carr. I trust that it will be published so that it may serve as a fundamental reference for students of learning. But your chairman has instructed me to say something extreme and belligerent.

For the sake of discussion, then, I will attack Carr's suggestion that the ways in which the after-effect of a mental connection work back upon it to strengthen it are multifarious. This attack will at least have the merit of bringing to your attention two sets of facts recently discovered which are important for any explanation of learning.

The first is the 'spread' or 'scatter' of the influence of a reward. A reward strengthens greatly the connection which it immediately follows, but it also strengthens slightly the preceding connection and the succeeding connection in the behavior series, and even connections two or three steps removed. In seventeen very different sorts of learning I found this universal, and the fact has since been confirmed by Muenzinger and others.

The second is the fact that increasing the amount of the reward beyond a certain minimum makes little or no difference to its potency as a strengthener. Let the learners be given, after a right response, sometimes one unit of money, sometimes two, sometimes four, and sometimes eight, within the same series of forty or so multiple-choice learning tasks of equal difficulty. The learning of those rewarded by eight units is little or no faster than the learning of those rewarded by one unit. A rewarded occurrence strengthens the connection five or six times as much as an unrewarded occurrence does, but large rewards do little more than small rewards.

¹³ I cannot guarantee that this statement, written from memory, is a faithful copy of what I said, but it probably represents the gist of it.

These two facts suggest that a single and simple mechanism (call it the confirming reaction or the O. K. reaction) may account for all the positive phenomena of the law of effect. This confirming reaction presumably consists in some force issued by the general psychological and physiological state of the organism. Its action is to reinforce, not sensations or movements, but connections. It reinforces chiefly whatever modifiable connection has just been in action, but, like other biological forces, it acts somewhat loosely and variably, and so spreads or scatters to neighboring connections. Rewards, successes, goal attainments and approaches, and satisfiers of all sorts exert their influence by evoking it.

There would then be two fundamental principles for learning.

(1) Any modifiable connection becomes somewhat stronger by occurring.

(2) A modifiable connection becomes much stronger if it or its accompaniments or after-effects evokes the confirming reaction. Observation shows that this is evoked by states of affairs which are satisfying to the animal in the sense that it does nothing to avoid them, usually doing such things as attain and maintain them.

Concerning the latter half of Professor Carr's statement, I suggest as possibly useful a supplementary classification of explanations into those which comfort us, those which are logically plausible and satisfy the criterion of consistency, and those which predict the course of events. The progress of the natural sciences has been largely due to its total rejection of the first sort, and its subordination of the second to the third.

IV

E. A. CULLER

*University of Illinois*¹⁴

Having no direct evidence to offer on the Law of Effect, I shall confine myself to a few considerations that seem worthy of attention.

Conditioning in my opinion is a kind of archetype of learning; the law of effect must therefore be considered in relation to both. A year ago at Hanover I sought to show that both learning and conditioning involve two separable concepts, whose relation may be presented by this simple schema:

Conditioning

- (a) Pattern of response—due to primary (unconditioned) stimulus
- (b) Energy which activates this response—provided also by primary stimulus

Learning

- (a) Pattern of response—due to primary stimulus
- (b) Energy for activating response—independent of primary stimulus; usually provided by social rewards or penalties.

In combining bell with shock-to-paw, we get conditioning. The primary stimulus (shock) is responsible for the conditioned pattern (flexing foreleg, instead of barking or wagging tail) and also for energizing this pattern. When shock is omitted, the pattern rapidly extinguishes since there is now nothing to activate it. But when Johnny learns the alphabet or practises his piano-lesson, these two functions are subserved by wholly separate agencies. The printed figures before his face determine the pattern of his learned behavior, whereas the incentive is provided by a piece of cake, say, or a spanking.

¹⁴ Communication No. 29 from the Physiological Psychology Laboratory (Animal Hearing), University of Illinois.

The two are just as distinct as the engine which propels a motor car and the man-at-the-wheel who determines its course. What has the Law of Effect to do with each of these?

1. It has nothing to do with laying down the primary pattern of behavior; that depends upon the stimulus-matrix (maze, problem-box, nonsense syllables). The law postulates that the animal has already acted before the 'effect' of its action becomes operative; hence the initial form of response is determined rather by the stimulus-complex than by the reward or punishment which follow.

2. It does have to do with activation; with determining which parts of the original design will be adequately energized and which will be allowed to lapse through lack of power to excite them. It seems to be a fact of nervous integration, whose mechanism is still obscure, that any pathway which *has just been* active remains hyper-excitable for a limited time (say a second or more); it can thus be re-activated by nerve-potentials which normally are subliminal. This recency-factor is of utmost import in the complex interplay of cortical potentials. It means that *back-effects* are, within this period of hyper-irritability, more than possible; they are inevitable. Their presence is well demonstrated by the material cited in Professor Thorndike's discussion. It is this factor which may underlie immediate memory; the events of the past few moments are, at any time, still vivid and alive. With repeated trials, this factor operates again and again and thus 'fixates' the response.

Of what nature is the response thus fixated? Here let us refer to Jennings' classic observations on certain protozoan forms. Paramecia, when swimming about in ordinary water, will react away from a drop of 0.1 per cent NaCl which thus remains quite empty; but when swimming in 0.5 per cent NaCl, they will rapidly accumulate in the drop of 0.1 per cent NaCl until it swarms with them. When Paramecium enters a more favorable region (low concentration of NaCl), it shows no detectible sign of change in its existing behavior; it swims along as though nothing had happened; but when it hits the opposite margin of the drop, it rebounds from the less favor-

able medium like a rubber ball. Through repeated reflections from the outer edge it thus keeps within the favorable area. In other words, the animalcule 'reacts' only to negative stimuli; positive ones are accepted without apparent change. In my opinion, every bit of behavior, from protozoon to man, can properly and usefully be reduced to some combination of these negative and positive elements (change and continuance, avoidance and acceptance, rejection and utilization). For these two opposed cases, the Law of Effect may be stated and 'explained' in two propositions.

(a) When any activity eventuates in a *more* favorable state, the transition from less to more favorable brings various changes throughout the organism. From these changes proceed streams of afferent impulses, which receive some affective toning in the thalamus and thus underlie the experience of satisfaction or well-being. Having reached the higher centers, these impulses cannot merely vanish; they must discharge. The paths most ready for discharge are those which *have just been* active and are still in the hyper-excitable period. Hence the acts just prior to reaching the food are *neurally re-enacted* for a moment in reduced degree, when the transition from hunger to food-taking begins; this automatic re-innervation of the acts leading to food helps to fixate them for the next opportunity. (b) When any activity eventuates in a *less* favorable state, the picture is quite different. Widespread organic changes again occur during the transition from more to less favorable; but they now consist of avoidance- and removal-reflexes, which notoriously take neural precedence over all other activity. We know how a negative (painful) stimulus crashes through an animal's existing patterns of behavior and supplants them with movements of escape and defence. The afferent impulses now streaming to thalamus and cortex do not, as in case (a), re-innervate the just preceding activity; rather they canalise into these vigorous negative reflexes, whose threshold of excitability is for the time lower than any other. The animal's dominant response to the less favorable objective

being negative, the movements leading to and culminating in it are correspondingly attenuated for the future.

Much more should be said on this difficult topic, but I must proceed to Professor Carr's questions about the nature and use of explanation. Scientific explanation has for its sole function to discover and describe the *mechanisms* by which things happen. As science learns more about the world, its descriptions become more adequate and systematic. It has nothing to do with 'cause and effect'; these are philosophic concepts, with no more perceptual validity or scientific import than cognate concepts like demon or god. All we need do and all we ever can do is to trace one by one the successive steps or operations by which one situation yields to another. Explanations may therefore be stated in widely different terminology and with variant degrees of completeness.

An explanation may thus be (1) *mentalistic* (introspective), wherein we describe the experiential links between the two terms, stimulus and response, in words like these: "The conscious field was suddenly dominated by clear green light. At once arose a momentary tension in tip of tongue (meaning *left*) attended by shifting pressure-masses about the eyeballs as they moved leftward. Then followed a fleeting visual image of the left hand and key; which in turn was supplanted by kinesthetic and tactile pressure-patterns in forearm, wrist and finger as the key was depressed." In explanatory terms, we might say that the proprioceptive patterns (signifying movement) were touched off by the visual image of the responding finger; this image in turn was evoked by pressures near the eyeballs; these pressures finally were aroused by perception of the green light. This mode of explanation, though legitimate as far as it goes, is inadequate. Experience (consciousness) is a *discontinuous* function of the living organism; thereby gaps are introduced (sleep, anesthesia) which can be bridged only with non-mental terms, which belong to a different universe of discourse. Accordingly, the (b) *behavioral* explanation has been widely adopted in preference to the mentalistic. Here one or more observers watch, either with the naked eye or by aid of various instruments,

what happens in a human or animal organism from the time a stimulus is impressed until a response occurs. Consider the cat-in-puzzle-box picture. "Upon being released, it moves slowly toward door, stops, mews, moves head from side to side, turns eyes toward a loop of string depending from top of box, seizes and pulls loop with right forepaw; door opens, cat walks out and seizes food." Why did the door open? Because the cat pulled the string. Why did it pull the string? Because it saw the loop within reach. Why did it see the loop? Because it was moving the eyes from side to side. Why did it move the eyes? Because its musculature was being generally activated by hunger impulses. This mode of explanation is commonly more useful than (a); but it seems undeniably superficial in contrast with the (c) *physiologic*, where the course from stimulus to response is more intimately traced. Our explanation here would be of this order: "Light energy emitted from a given source, upon passing the refractive media of the eye, forms a circular pattern about the fovea, and by effecting certain chemical transformations in the receptive cells, initiates nerve-impulses via optic tracts, lateral geniculates and optic radiation to the occipital cortex. Certain changes in cortical potential then occur, whose locus and pattern are still obscure, which eventuate in efferent impulses to the flexors of right forelimb." Inasmuch as learning and conditioning involve physiological mechanisms, this type or level of explanation seems to me far more useful and significant than either (a) or (b). We are here dealing with the actual mechanics of the organism, comparable to the 'works' of a clock. Whereas the behaviorist observes the clock from without, noting how its hands change their relative and absolute position, the physiologic psychologist seeks to examine the organic structure in its functional relations. This direct examination of the inner mechanism tells us far better why and how the hand moves from 11 to 12 than does any amount of external observation.

The next level is (d) *biochemical*, where the units or operations of (c) are further reduced. Thus, 'nerve-impulse' is one step in the physiologic explanation; at the biochemical

level we inquire in turn: of what processes is the impulse itself constituted? The biochemic analysis begins where the physiologic leaves off. If we wish to be speculative we may say that a final (or is it final?) level remains: the *atomic* (*e*). The biochemistry of (*d*) proves to consist of certain goings-on within the atom, statable in terms of protons and electrons and their interaction. This level may, however, well be disregarded as an actual form of biologic explanation.

V

J. F. DASHIELL

University of North Carolina

Could we but cast off our theoretical biasses, surely we would all acknowledge that *in some sense* a process of learning is modified definitely by the consequences incurred. Concerning what Dr. Carr has named the empirical law of effect there does indeed exist "a high degree of unanimity of opinion that the food and the shock do determine to a large extent the final product of the selective and eliminative process" [question 1].

Further, I believe, our divergencies do "arise at the point of explanation and especially . . . the theoretical type of explanation" [question 5]. Divergencies arise when the Law of *Effect* is assumed to be a Law of *Affect*, *i.e.*, that the consequences of an act that are important in modifying the act are always, or at least typically, *conscious* consequences. The historical perspective is familiar. When first stated explicitly by Bain in the 1850's, the theory of learning was writ quite completely in terms of conscious experience. Suppose one of the learner's random movements happens to bring him an experience of pleasure; then the memory of the pleasure becomes associated to the memory of the movement and so helps in the rearousal of the latter. (We must remember that Bain was still influenced by the Associationist doctrines, *i.e.*, association of ideas.) Nowadays, of course, the notion of the memory of the movement, has dropped pretty well out of the picture of learning. Is the memory of the pleasantness or unpleasantness similarly dropping out? (It seems that many cases of learning are not statable in such terms, as for instance, in the case reported by Baker in which the pupillary response is conditioned to a subliminal stimulus.)

In his fourteenth question, Dr. Carr suggests that the psychological type of explanation may be better than the

physiological type. Granted! As Köhler once said, "Between the stimulus and the response there is more terra incognita than there was on the map of Africa sixty years ago"; and one thing that psychologists are awakening to is the validity of descriptions of phenomena on the *psychological* level purely, without necessity of recourse to neurological facts, real or suppositious. But is the psychological plane limited to the experiential plane? Dr. Carr's own researches in animal behavior have not been experiential, yet they have not been physiological, either.

May I suggest that the challenges hurled at Dr. Thorndike's statements of the Law of Effect have come in two forms? It seems that Dr. Carr is addressing his queries especially at the critics of the *hedonic* phrasing of the Law. But another type of criticism takes the form: how can a consequence affect its cause; or, how can a later happening modify its antecedent, or even a tendency to repeat the antecedent? If there is a backward action—and we must grant that there seems to be—how can this be explained?

Finally, I would like to suggest a reorientation. I can point it by referring to Dr. Muenzinger's well-known experiment (which is being confirmed by some experiments on human subjects in the North Carolina laboratory) in which shocking for the right choices was as efficacious as shocking for the wrong. We have let ourselves be surprised at this 'shock-right' phenomenon; but the surprise is due to our having emphasized the word 'shock' and not the word 'right.' But what does 'right' mean? We have been attending to too small a section of fact. We have not considered that this act of choosing one or the other pathway is only a part of a larger behavior-act. The rat was trying to get somewhere, to get something; and if the shocking paths led to that something, there we have a more basic fact than merely that they shocked. Which, in this case, I ask, is the effect contemplated by the Law of Effect: is it the shock effect or the right-path-to-objective effect? (This matter is illustrated, I believe, in the report made by Honzik and Tolman¹⁵ showing

¹⁵ C. H. Honzik and E. C. Tolman, An experimental study of the effect of punishment on discrimination learning (abstract), *Psychol. Bull.*, 1937, 34, 775.

that the efficacy of punishment attached to right responses increases as the proximity of the punishment to the choice doors.)

Putting my suggestion in another way: Is not the effect that is important in learning a function of the amount of organization of the animal or man at the time, or at least of the amount of organization of its behavior at the time? A starfish or earthworm would be more obviously trained by mere shocks and good-tastes than a chimpanzee, a child more than a man, a man in a disorganized state of drowsiness or fatigue than when in full possession of his faculties.

VI

KARL F. MUENZINGER

University of Colorado

In response to Mr. Carr's paper I would like to list some points of agreement, discuss an important disagreement, and offer some suggestions about the formulation of empirical laws of effect.

1. I agree that the object of science is agreement, and that disagreement is valuable only in so far as it leads to further research.

2. I agree that explanations may be of different kinds and on different levels. Especially important for a discussion of a law of effect is Mr. Carr's distinction between first level or empirical and succeeding level or theoretical explanations.

3. I agree that the merit assigned to an explanation depends upon one's standard of reference, and that "misunderstandings would tend to disappear if we were careful to specify the nature of the standard employed." (This is the Chicago tradition of functionalism at its best.)

4. I also agree heartily with Mr. Carr's analysis of the use and misuse of the term 'objective.'

There is one statement with which I disagree. Mr. Carr questions the value of the desire for explanatory simplicity and asks "why may not the mechanism of connection vary with the organism such as a rat or a human being?" Using as my standard of reference systematization, I value a single explanation higher than a multiplicity of explanations for the same set of phenomena. The empirical relations which we can observe in a rat's behavior certainly differ from those in human behavior. But the theoretical principles of behavior which we want to invent must be the same for rats and human beings. I would certainly stress the fact that the effect of shock varies with rats and human beings,¹⁶ but I would also insist that the

¹⁶ K. F. Muenzinger, Motivation in learning. II. The function of electric shock for right and wrong responses in human subjects, *J. Exper. Psychol.*, 1934, 17, 445.

same law of effect is operative in rats and human beings alike. We would, of course, also have to make the third level assumption that special masking factors produce the phenomenological dissimilarities constituting the facts of observation with which we start.

Mr. Carr suggests that there might be a high unanimity of opinion as regards an empirical law of effect, namely, that food and shock determine the course of learning to a large extent. And he further claims that divergences arise when we reach the theoretical or second and third levels of explanation. This, we may admit, is a factual statement of the case. We do not disagree about the effect of food—we are careful to see that our hungry rats will find food in the apparatus—but we disagree about the assumed mechanism by which the food-reward determines the behavior of the rat on subsequent trials.

Illustrating such disagreement, Mr. Carr refers to Mr. Thorndike's claim that it is the affective element in the after-effect of a response that influences a connection. Here, it seems to me, we might restrict ourselves, as Mr. Carr has suggested elsewhere,¹⁷ to the observable relations of the *success* of a response to the probabilities of its recurrence. Then we might ask: What are the present possibilities of formulating empirical laws of effect, laws whose prime requisite must be that they rest on experimentally verifiable relations?

Taking into account Mr. Thorndike's discovery of the spreading effect of reward,¹⁸ or as I would say, of success, I would suggest as the first half of a *general* empirical law of effect a statement like this: *Not only does a successful response itself tend to be repeated but also the contiguous responses before and after it.* Or, worded as a formula: *Success produces a double gradient of uniformity.* Either one of these statements is merely a paraphrase of Mr. Thorndike's own summary of experimentally verifiable facts. The purpose of my paraphrasing is to use a language that is devoid of theoretical implications.

¹⁷ H. A. Carr, *Psychology*, New York: Longmans, Green, 1926, p. 92.

¹⁸ E. L. Thorndike, *An experimental study of rewards*, 1933.

Being accustomed to a dichotomy in the law of effect one is immediately led to ask: How about the effect of failure? You are familiar with Mr. Thorndike's abandonment of the original dichotomy in the orthodox Law of Effect. Whereas formerly the annoying component of an after-effect was supposed to weaken a connection, just as a satisfying component was supposed to strengthen it, he now says that an annoyer is not the dynamic opposite of a satisfier.¹⁹ His facts are these—that whereas a successful response is repeated with a frequency well above chance, an unsuccessful response is not repeated with a frequency well below chance, but with a frequency still better than chance, although only slightly so.²⁰ I do not question the facts themselves. But I do question their validity for the generalization which Mr. Thorndike makes. Several experiments recently performed in the Colorado laboratory have shown that Mr. Thorndike's statement of the facts is correct, but incomplete. Two additional facts can be demonstrated: First, that an unsuccessful response reduces the frequency of repetitions of contiguous successful responses, just as a successful response increases the frequency of repetitions of contiguous unsuccessful responses.²¹ Second, that given a similar mental set towards unsuccessful responses as there naturally exists towards successful ones, the unsuccessful response is repeated with a frequency well below chance.²² Taking into account these facts, I would propose as the second half of a general empirical law of effect a statement like this: *Failure tends to vary a response and also the contiguous responses before and after it.* Or, worded as a formula: *Failure produces a double gradient of variability.*

¹⁹ E. L. Thorndike, *Fundamentals of Learning*, New York: Bur. Publ. Teachers Coll., Columbia Univ., 1933, p. 313.

²⁰ E. L. Thorndike, Reward and punishment in animal learning, *Comp. Psychol. Monog.*, 1932, 8, Serial No. 39.

²¹ K. F. Muenzinger and C. C. Dove, Serial learning: I. Gradients of uniformity and variability produced by success and failure of single responses, *J. Gen. Psychol.*, 1937, 16, 411.

²² C. C. Dove, The effect of different degrees of success and failure upon uniformity and variability of behavior, *Comp. Psychol., Monog.* (in press).

So far my generalizations have been concerned with the after-effects of success and failure. What about the effects of annoyers as such? Taking into account our recent experiments on electric shock for right and wrong responses²³ and on obstacles,²⁴ I would suggest the following statement as one among probably numerous *special* empirical laws of effect: *Any condition after the point of choice which tends to make the animal pause at the point of choice also tends to facilitate learning.*

There is evidence that the occurrence of a painful stimulus, such as electric shock, under certain conditions, as for instance before the point of choice, may have a disrupting effect.²⁵ If confirmed, such facts would furnish the basis for another specific law of effect.

These statements I submit as samples of generalizations of empirical relationships ('laws,' if you like) capable of experimental demonstration, samples on which agreement should be possible by reference to facts, samples to which no doubt others can be added.

²³ K. F. Muenzinger, Motivation in learning. I. Electric shock for correct response in the visual discrimination habit, *J. Comp. Psychol.*, 1934, 17, 267; Muenzinger and A. Wood, IV. The function of punishment as determined by its temporal relation to the act of choice in the visual discrimination habit, *J. Comp. Psychol.*, 1935, 20, 95; Muenzinger and F. M. Fletcher, VI. Escape from electric shock compared with hunger-food tension in the visual discrimination habit, *J. Comp. Psychol.*, 1936, 22, 79.

²⁴ K. F. Muenzinger and H. Newcomb, Motivation in learning. V. The relative effectiveness of jumping a gap and crossing an electric grid in a visual discrimination habit, *J. Comp. Psychol.*, 1936, 21, 95; Muenzinger and F. M. Fletcher, VII. The effect of an enforced delay at the point of choice, *J. Comp. Psychol.*, 1937, 23, 383.

²⁵ K. F. Muenzinger and A. Wood, Motivation in learning. IV. The function of punishment as determined by its temporal relation to the act of choice, *J. Comp. Psychol.*, 1935, 20, 99.

THE SYNTHETIC SENSE AND INTELLIGENCE¹

BY THOMAS V. MOORE

The Catholic University of America

If we look at the theory of perception as developed in philosophy since the days of Descartes we find in general an attempt to account for knowledge as a sensory presentation to the mind of objects that act upon the sense organs. The sensory presentation is sometimes conceived of as being fortified by similar sensory images that owe their possibility of recurrence to the fact that at one time they occurred as sensory presentations just as the object now being perceived is being presented by the senses.

The modern school of Gestalt-psychology has brought forward a vast amount of evidence that shows the inadequacy of this naïve philosophical sensationalism, and has shown in various ways that one must take into consideration the total sensory impression with its various relative values of one item as opposed to another in the complex sensory presentation.

But Gestalt-psychology, in its modern form, is not an escape from sensationalism, but a mere modification and development of a philosophy which is unwilling to allow psychology to launch out into the deep and investigate regions of experience that are beyond the possibilities of sensory interpretation.

The present study is an attempt to lay aside any limitations imposed by Gestalt-psychology, and investigate the data of pathology for any light it may throw on the analysis of the mind and the nature of perception.

In this investigation we will be guided by a rule the intrinsic reasonableness of which will be apparent to anyone after due reflection. This rule of pathology may be expressed as follows:

¹ Read at the Nottingham Meeting of the British Association for the Advancement of Science, September 2, 1937.

Whenever a single function is destroyed or profoundly affected after a certain type of pathological change:

(a) The function affected is in some manner connected with the normal activity of the tissue that has undergone pathological change.

(b) The function affected must be recognized as in some manner different from the functions that remain relatively intact.

If one turns to the literature one can by this rule make a very good beginning in the analysis of mental life. One may distinguish clearly between memory and perception. The data of the senses are not stored in the mind by the very fact that they are perceived, as a stamp is imprinted on a document. If this were the case, everything perceived would be remembered. But many pathological cases demonstrate the fact that one may correctly perceive and interpret the facts of the present situation, but even in a few seconds no memory of the data of perception remains in the mind.

But we are interested at present in the pathology of perception and the light it throws on the involved sensory and intellectual processes.

Just as pathology breaks up memory into the complementary functions, storing and recall, so it breaks up perception into several different processes.

To give a summary view of the pathology of perception we may say that there are:

(a) patients who can receive and give an account of the sensory qualities derived from the perception of an object and have nevertheless no idea of what the object can possibly be. Perception, therefore, does not consist merely in the reception of sensations;

(b) patients who can configure sensory qualities and still have no concept of what the object is. Configuration, therefore, does not exhaust the concept of perception;

(c) patients who can configure the parts of a picture but cannot interpret the picture as a whole;

(d) patients who can configure and interpret a single

picture, but are unable to arrange a series of pictures in their logical order.

Let us consider briefly a few examples and then pass on to the meaning of these pathological conditions.

A number of cases have been reported where patients describe an object felt,² or chewed in the mouth,³ or looked at,⁴ so that one can see from their description that they perceive sensory qualities, but do not know what they are perceiving. The various forms of what is termed sensory aphasia manifest this disorder in the auditory field. It seems that any sense may deliver in consciousness its specific sensory quality, but that this awareness of the data of sense does not constitute perception; and gives no meaning, no knowledge of what is being presented to consciousness by the activity of the organ of sense.

The *Gestalt* psychologist would be inclined to point out that this is just what one would expect. Non-configured sensory data do not constitute perception. That is true, but it is also true that configured sensory data are also inadequate, of themselves alone, to give any knowledge of what the object is. It is seldom that these patients are unable to make any configuration at all. The attempt to perceive and interpret is an effort to them. Persistence may make the patient for the time being totally color blind,⁵ all configuration disappears, and he is conscious only of a weird mass of sensory data.

But usually the patient sees or feels various configured elements. Thus the patient may pick out the windows in the picture of a cathedral or point to the pinnacles and say they are steep like a roof and still be unable to say what the picture represents.

² E.g. A. Gans, Ueber Tastblindheit und über Störungen der räumlichen Wahrnehmungen der Sensibilität, *Zsch. f. d. ges. Neurol. u. Psychiat.*, 1916, 31, 305. Good references to the previous literature.

³ Raissa Goland-Ratner and J. Rosenblum, Ueber einem seltenen Fall von agnostischen und apraktischen Störungen mit anatomischer Befund, *Arch. f. Psychiat.*, 1932, 97, 569-599.

⁴ Joh. Stein and H. Bürger-Prinz, Funktionswandel im Bereich des optischen Systems, *Deutsch. Zsch. f. Nerven.*, 1932, 124, 189-214.

⁵ Cf., Stein and Bürger-Prinz, *loc. cit. supra*.

At times a patient may be able to look at an object and draw it, so that, for instance, one may see the round body of a watch, with the ring above to attach the watch-chain, and the fluted head of the stem winder; and still, though the patient evidently sees a configured presentation, picks out its characteristic details, and draws the whole and its parts so that a normal person may recognize the drawing, he himself is unable to interpret his own drawing or explain or indicate in any way what it is that he has seen and drawn.⁶

The more one studies the defects in the sensory synthetic basis of perception, the more one is surprised to find through how many stages it passes and how wide are its spatial and temporal ramifications in the mechanisms of the mind.

We are all familiar with the stages of children's reaction to pictures made use of by Binet in his measurement of children's perceptions. But the adult mind after years of experience may develop an organic pathological condition in which it can see individuals in a picture and describe what they are doing and yet fall into the most glaring absurdities in the attempt to interpret the picture as a whole.⁷

One would think that the ability to interpret one picture should enable a patient to see a series of pictures in their proper logical sequence;⁸ or that if one could understand the propositions of a syllogism he should always be able to grasp them as a whole and draw the necessary conclusion.⁹ But the surmise is not correct. The elements of a series must be held simultaneously before the mind, only one indeed at a time in focal awareness, but the others must not be lost entirely. The conscious and the unconscious, the present and the immediate past, constitute a complex which must be held together in a sensory synthesis, if the parts are to be understood in relation to the whole, and the complexities of the

⁶ H. Lissauer, Ein Fall von Seelenblindheit nebst einem Beitrage zur Theorie derselben, *Arch. f. Psychiat. u. Nervenk.*, 1890, 21, 222-270.

⁷ I. Wolpert, Die Simultanagnosie-Störung der Gesamtauffassung, *Zsch. f. d. ges. Neurol. u. Psychiat.*, 1924, 93, 397-415.

⁸ Cf. W. Van Woerkom, Über Störung des Aufgabebewusstseins in einem Fall von Tumor des Frontalhirns, *Monatsschrift f. Psychiat. u. Neurol.*, 1928, 70, 59 ff.

⁹ *Loc. cit.*, p. 63.

meaning and its various implications are to be apprehended in an intellectual nonsensory interpretation.

In the field of tactual sensation one may find touch, temperature-sense, pain, two-point discrimination, localization, kinæsthesia, and vibration sense all normal; and still the patient will handle a key, feel that it is something cold, surmise that it is a metal object, recognize it is a long object with something projecting at the end and be utterly unable to tell what it is till it is placed in the normal hand and then he says at once: a key.

Here we have evident tactual configuration of the object as a whole and of its various partial constituents. But just as one patient can see items in a picture, and be unable to interpret the whole; and another can interpret the individual pictures of a series, but is unable to put together the story they represent, so this patient perceives and interprets various elements of a tactual complex, but is unable to bring the fragments together in relation to past information and recognize by touch the object he is holding in his hand.¹⁰

The study of patients operated on late in life for congenital cataract throws further light on configuration and meaning.

Does the optical system without experience present to the mind a configured or an unconfigured world? One would think that the author of 'Die physischen Gestalten' would answer this question by a definite affirmation of natural and spontaneous configuration. In fact he seems to do so. Thus he writes: "We know that the retina develops electromotor forces in its activity, and we have already recognized their configurational properties. Therefore there are found genuinely configured occurrences in what we term the peripheral eye."¹¹ But he later entered a definite disclaimer and categorical denial. "We have no organization at all in retinal stimulation, no wholes, no groups, no segregation."¹²

¹⁰ F. Rose and M. Egger, *Steréognosie et asymbolie tactile*, *La semaine médicale*, 1908, 28, 523.

¹¹ W. Köhler, *Die physische Gestalten*, Erlangen, 1924, p. 197.

¹² W. Köhler, *Gestalt psychology*, New York: Liveright, 1929, p. 176. Cf. also *Psychologische Probleme*, Berlin: Julius Springer, 1933, pp. 109 ff.

And yet when a patient has been operated on late in life for congenital cataract and all at once comes into the use of vision, he at once sees a configured world. One can hold up before him an object with which he has been familiar by years of experience, for example his hat or his valise. He points to it. He traces its outline with his finger in the air. Evidently he sees a Gestalt or configuration of visual stimuli. A definite object is differentiated from the background. But the configuration gives him no meaning. He has no idea of what it is. The object is placed in his hands—he recognizes it. He exclaims with surprise. Can it be such a thing!¹³

Even before the patient leaves the operating table he sees an object, the hands of the physician, and exclaims: "Doctor I see. In the midst of the blue I see something still more blue. It seems to be moving. I think it must be your hand."¹⁴

Configurations alone have no meaning whatsoever but they are given of themselves with normal vision. It is sometimes possible for the patient to use previous information and *deduce* in this way the meaning of a configured impression, as in the case just mentioned. A more evident deduction, though a demonstration of false logic, is that of the patient who saw the wall and something on the wall, and correctly, though illogically, concluded that what he saw was a picture because he had heard that pictures hang on walls.¹⁵ Evidently the differentiation of picture and wall involved the perception of figure and background. But the configuration of the impression gives no meaning. And contrary to the position of Gestalt psychology the meaning is derived not from the configuration but from past experience.

One might perhaps be inclined to say configurations alone mean nothing. Sensations alone mean nothing. What we need is to unite the concepts of the old sensationalism with

¹³ Ferrer, Ciego de nacimiento operado con éxito a los 29 años, *Revista Medica Cubana*, 1924, 35, 829.

¹⁴ *Loc. cit.*, p. 828.

¹⁵ R. Latta, Notes on a case of successful operation for congenital cataract, *Brit. J. Psychol.*, 1904, 1, 140.

the data of the new configurationalism, and point out that meaning depends:

- (a) on the configuration of sensory qualities into a unit of sensory experience;
- (b) on the awakening of a mental image of past experience. When such an image is awakened by a configurational process we have meaning. When it is not the configuration is a meaningless sensory presentation.

This concept has long been ruled out by a study of the temporal relations of meaning and imagery.¹⁶

Meaning comes in the process of perception before revived imagery. What comes before in a temporal sequence is neither identical with nor dependent upon that which comes after.

Furthermore, meaning is often present when imagery does not appear at all and it has in itself the universal characteristics of a definition which cannot be painted and perfectly pictured in sensory colouring.

Nor can we find escape in the position of such logicians as Bradley and Bosanquet,¹⁷ who split the mind into two compartments, the logical and the psychological; and store away meaning in the logical and images in the psychological, admitting no possibility of transit from one to the other. For after all, there is but one mind and the psychology of logical processes is just as truly psychology as the study of sensation and sensory memory.

From all this it seems that we must introduce a chapter into psychology that has been lacking since the days of Descartes: the chapter on the synthetic sense.

¹⁶ T. V. Moore, The temporal relations of meaning and imagery, *Psychol. Rev.*, 1915, 22, 177-225; Image and meaning in memory and perception, *Psychol. Monog.*, 1919, 27, No. 119.

¹⁷ Thus Bradley: "But an idea, if we use idea of the meaning is neither given nor presented but is taken. It cannot as such exist. It can never be an event with a place in the series of time or space. It can be a fact no more inside our heads than it can outside them," in 'Principles of Logic,' 1883, p. 8. But it is possible to react both to meaning and imagery. Therefore both meaning and imagery have a place in a temporal series. See T. V. Moore, *loc. cit. supra*, also Agnes R. McDonough, The development of meaning, *Psychol. Monog.*, 1919, 27, No. 122.

The evidence we have mentioned in this paper is but a meagre sampling of a vast body of observations that point unmistakably to the fact that sensations must be synthesized in order to be interpreted. The sensation is one type of psychological experience and the intellectual interpretation is another. But the mind is unable to interpret the data of sense unless such data undergo a sensory synthesis which unites present and past sensory experience in such a manner that interpretation is possible.

What we see, for instance, appears to us normally as a certain distance away without our having to ponder over the matter at all. But this feeling for distance is absent in the patient who has been blind from birth and has sight suddenly bestowed upon him by an operation for congenital cataract.¹⁸ Every perception takes its place in a temporal sequence of mental events which as they occur do not drop suddenly into oblivion but are held together in an act of sensory synthesis. All perception involves a temporal and spatial sensory complex. Daily life is making continual demands on past experience; and both intellectual and sensory elements of past experience must be related to the temporal and spatial complex that dominates consciousness as it experiences the present moment. Isolate the present moment from the influences of past experience and what is presented to consciousness becomes so strangely different that recognition and interpretation become impossible. This is the patent fact that constitutes the important lesson of the agnosias. The present results of sensory stimulation even from one and the same sense organ are inadequate material for cognitive interpretation.

Only by a sensory synthesis can sensations become knowable. This synthetic activity is logically different from both sensation and memory and pathology points out its physiological independence. It merits special study, more than memory or the sensations themselves. It is perhaps the main factor apart from breadth of experience, by which the mind

¹⁸ Cf., the references in Thomas Verner Moore, Gestalt psychology and scholastic philosophy, *The New Scholasticism*, 1934, 8, 55 ff.

of the child differs from that of the adult and the intelligence of a genius transcends the intellectual capacity of the moron. It is particularly the synthetic sense that degenerates in the organic dementias.

The synthetic sense is a *sine qua non* for the passing of every mental test; and is often the main factor differentiating the abilities of the individuals who constitute the tested population. It makes the main contribution to what is Spearman's statistical general factor in intelligence.

It is in itself a complex group of functions. It involves both sensory analysis and sensory synthesis, for to see the important elements in a complex they must be isolated from their background; and to realize that they express that which is fundamental and essential the products of analysis must be apprehended as a synthetic unit. The failure to analyse and the crumbling of every attempted synthesis gives us the picture of the agnosias. An agnosia is fundamentally a sensory defect rather than a disorder of intelligence itself. But the sensory defect is not that of the sense organ or peripheral sensations, but is due to a central lesion which affects the synthetic sense.

And withal, though unmentioned in our text books of psychology, it is not new. The Platonic Socrates almost crystalizes its definition in the *Theataetus*. But it was Aristotle who recognized the necessity of a sensory synthesis in the act of perception and first described the function of the *κοινή αἴσθησις*, the synthetic sense, the *sensus communis*.

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PHYSIOLOGY, PSYCHOLOGY AND SOCIOLOGY

BY EDWARD CHACE TOLMAN

University of California

I would define physiology as a study of the laws determining the *activities* of muscles and glands; I would define psychology as a study of the laws determining the *behavior* of whole organisms; and I would define sociology as the study of the laws determining the *conduct* of groups of organisms.¹

Accepting these definitions, one's first reaction concerning the interrelations of the three sciences would be to think of physiology as the most basic, psychology as the next most basic, and sociology as the least basic—or, in other words, to conceive the facts and laws of psychology as dependent upon those of physiology and the facts and laws of sociology as dependent upon those of psychology. But the thesis that I am actually going to try to uphold here is the reverse and, at first sight, seemingly absurd one, to wit: that the facts and laws of psychology are, rather, in some part dependent upon those of sociology and that the facts and laws of physiology are similarly in some part dependent upon those of psychology.

It was the Gestalt Psychologists who first brought it home to psychologists in America that even within the confines of a single science, such as psychology by itself or physics by itself, the whole can often be said to govern its parts quite as truly as the parts may be said to govern the whole. Hence what I am going to argue for is, in a sense, merely a further extension of this Gestalt doctrine to be applied to the interrelationships between the three successively larger sciences of physiology, psychology, and sociology.

But the whole argument will be made clearer perhaps by basing it on a concrete example. The situation I have chosen

¹ This respective use of the three words, 'activities,' 'behavior,' and 'conduct,' was suggested to me by Professors K. F. Muenzinger and R. H. Bruce.

is relatively banal but it will, I hope, do for the purpose. As an example of *group conduct* I have taken the 'carrying out of a marriage and wedding by a middle-class American social and religious group' some twenty or thirty years ago—at a date, that is, when there were such groups and when marriages and weddings were expressions of them in a way that is probably less true today. As an example of *individual behavior* within this group conduct I have picked the behavior of a certain young man *C* in 'going to this wedding.' And, finally, as examples of *physiological activity* I have chosen the parallel 'muscle contractions and gland secretions' occurring in *C* in going to the wedding and in particular at the moment *M* when he encounters, on his way to the church, the family of a bridesmaid in whom he is 'interested.'

Let us indicate the interrelations between these three levels of performance and the explanatory concepts which I would conceive as respectively involved in them by means of a diagram.

In Fig. 1 physiology has been put along the bottom, psychology in the middle, and sociology at the top. Further, it has been assumed that each of these sciences operates with three classes of variable, to wit, *independent variables*, *intervening variables*, and *dependent variables*.

Independent variables are to be defined as those which the science in question manipulates in direct experimental (or historical) fashion and which it assumes as the ultimate causes of its phenomena. These independent variables have been put towards the left on each level of the diagram. The final *dependent variables*—i.e., the 'group conduct,' the 'individual behavior,' and the 'physiological activities,'—which are ultimately to be predicted and, in so far as possible, controlled have been put at the right on each level. And, lastly, the *intervening variables* are to be conceived as further constructs which each science finds it helpful to invent and to introduce as explanatory steps between the independent variables, on the one hand, and the final dependent variables, on the other. These have been put in between on each level. Finally, it must be noted that for each science there have also been

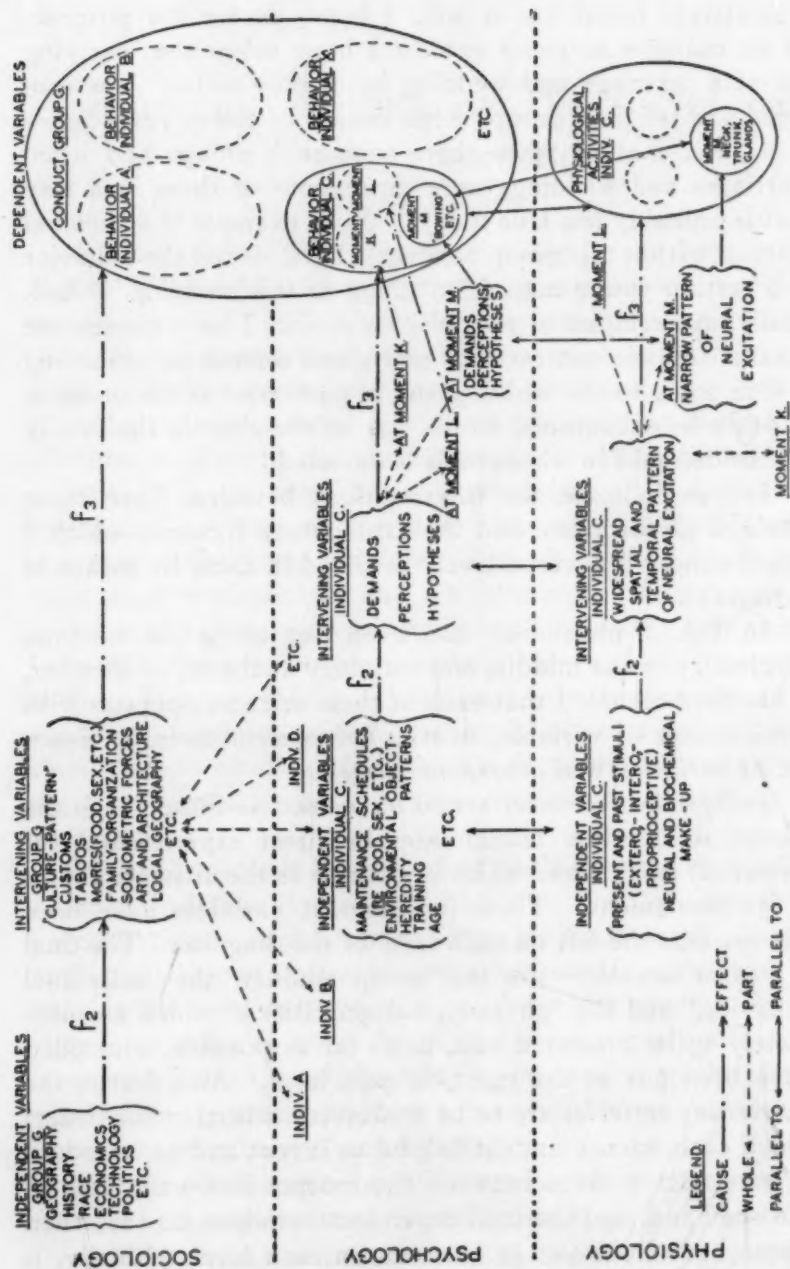


Fig. 1

inserted two sets of functions, indicated by dash-dot arrows (see legend), viz.: f_2 functions whereby the intervening variables result from the independent variables and f_3 functions² whereby the final dependent variables result from the intervening variables.

Look now in more detail at the right hand side of the diagram and consider, first, both the sociological and the psychological dependent variables. The 'group conduct' to be predicted is that of the whole group G in the carrying out of this marriage and wedding. This conduct I have represented by the large unbroken ellipsoid figure extending over both the sociological and the psychological levels. Within this larger ellipsoid I have indicated a series of smaller ellipsoids. Each of these latter is to represent a 'behavior' (relative to the wedding) of some one of the component individuals in the group. One of these, the unbroken-line ellipsoid, represents the behavior of individual C . Further, it will be noted that within this total behavior of C in 'going to the wedding' there are also to be mutually distinguished a number of still smaller, component behaviors represented by smaller included ellipsoids. And among these smaller behaviors is that of 'stopping, bowing, chatting, etc.' occurring at the moment M in which C encounters the family of the bridesmaid.

Look, now, at the physiological dependent variables represented on the bottom level. It will be observed that I have assumed a parallelism between physiological activities and psychological behaviors, not the older psycho-physical or psycho-physiological parallelism but a simple behavior-physiological parallelism. And this parallelism I have represented by double-headed solid-line arrows (see legend). Furthermore, just as the smaller behaviors involved at moments K , L , M , etc., were to be conceived as contained within the larger, more total behavior of 'going to the

² What I would designate f_1 functions (in contrast to these f_2 and f_3 functions) would be the complete functions stretching all the way from the independent variables, on the left, to the final dependent variables, on the right. If such f_1 functions were entirely known, there would be no need of the intervening variables. The latter and the f_2 and f_3 functions would be all absorbed into these f_1 functions. See Tolman (8, 10).

wedding,' so the smaller complexes of physiological activities at moments *K*, *L*, *M* are in like fashion to be considered as comprised within that larger loose complex of physiological activities which parallels the total behavior.

Let us consider, now, some general features concerning these three types of dependent variable. First, it is to be observed that the 'group conduct' is the most extensive both temporally, spatially, and inter-individually. It covers all the preparations for the wedding (*e.g.*, the sending out and receiving of invitations, the sending and receiving of wedding presents, the decoration of the church, and the like), and indeed even the original rearing and bringing together of the two young people in question, in addition to the wedding proper. It is such (spatially, temporally and inter-individually) extensive group-processes which the sociologist, is interested in predicting and, if he be an applied sociologist, of eventually learning to control—*i.e.*, learning to prevent, to modify, or to cause to be repeated unmodified through successive generations (in the same or other groups).

Any single 'individual behavior' such as this one of *C* in 'going to the wedding' is temporally and spatially less extensive and it inheres in but a single individual. It is one of many behaviors by one of many individuals, all of which are involved in the total group conduct. It extends over only a relatively small number of hours and a relatively small expanse of space. And, further, any such behavior can often be subdivided into a number of still smaller but distinguishable component behaviors. Each of these latter extends over minutes only. Finally, it may be either the larger more inclusive behavior or one of these smaller component behaviors which in the given case the psychologist is interested in predicting.³

Turning to the 'physiological activities,' we note that there is no single complex of physiological activities to be coordinated with the total group conduct. Physiological

³ This problem of superordinate and subordinate behaviors—*i.e.*, of means-end hierarchies—is one requiring more study and attention than it has yet received. For a previous attempt at discussing it, see Tolman (7, Chap. VI).

activities *per se* are expressions of individual organisms. There is no physiology for a group as such. Within the individual organisms there do seem to be, however, more and less extensive, more and less enduring, arrays of muscle contractions and gland secretions—parallel, respectively, to the more inclusive and the less inclusive 'behaviors.' Thus it may be either the larger or one of the smaller physiological complexes which the physiologist is interested in predicting and controlling. Actually, however, it is my guess that (in the present state of our knowledge) it is usually only one of the smaller complexes which a physiologist in any given case is attempting to predict and control.

Let us consider, now, the independent and the intervening variables at each of the three levels.

Sociology.—Turn to the upper left hand corner. Here I have put what I conceive to be the independent variables for sociology.⁴ They are the geographical, historical, racial,⁵ economic, technological and political factors which underlie and condition Group G and its conduct. They are the ultimate determiners of this group and of such particular conducts carried out by it as that of the present marriage and wedding. It is these geographical, historical, racial, economic, technological, and political causes which would have to be changed if Group G were to be other than it is and if it were not to 'go in for' this present marriage and wedding.

In attempting, however, to work out the functional connection between these ultimate determiners and any final group conduct, the social scientist is led to construct certain hypothetical 'intervening variables.' That is to say, instead of attempting to state at once the complete (f_1)⁶ functional connections between any given conduct (such as the present

⁴ It will be obvious to any *bona fide* social scientist that my notions as to the specific concepts of the social sciences are relatively naive. I venture to hope, however, that my conclusions as to the interrelations of them to the other two sciences will nonetheless still hold.

⁵ I am using 'racial' here in a loose sense to refer to the hereditary biological constitution of a group (due to inbreeding or what not) in so far as this heredity makes this group different, if it does, from other groups. See Linton (4, Chap. II).

⁶ See above footnote, p. 231.

wedding), and such geographical, historical, racial, economic, technological, and political causes, a sociologist today seems to find it more feasible merely to trace such conducts back, first (f_1 function), to a set of 'intervening variables' such as the customs, taboos, religious and sex mores, types of family-organization, 'statuses,'⁷ 'sociometric forces,'⁸ art and architecture, local geography, etc., of the given group. A most useful single term to cover these intervening variables would seem to be that of the 'culture-pattern' suggested by Benedict (1). Only after having invented this intervening variable of the culture-pattern, does the sociologist attempt to find a further set of laws or functions (f_2) to connect such a culture-pattern to the final independent variables of geography, history, genetics, economics, technology and politics. The social sciences break up the total relation into two successive steps—the f_2 laws and the f_3 laws. The f_2 laws as now stated seem to be relatively simple. They are hardly more than descriptive exemplifications of the culture-patterns. The f_3 laws, on the other hand, are hardly known at all. Anthropologists and sociologists are apparently as yet finding great difficulty in saying much of anything with regard to the f_3 's.⁹

Psychology.—Turn now to psychology. The sort of independent variables, which, as psychologists, we assume as determinative of the behavior of individual C, I have located in the diagram directly under the sociological intervening variables for the group as a whole. These psychological independent variables are: 'maintenance-schedules' (*re* food, sex, sleep, etc.), 'environmental object-patterns,' 'heredity,' 'training,' and 'age,' etc. And I have drawn a double-headed dash arrow (see legend) between these psychological

⁷ See Linton (4, Chap. VIII).

⁸ See Moreno (6).

⁹ As an example of an attempt to discover such f_3 laws, see the studies edited by Margaret Mead (5). This survey of thirteen primitive cultures sought for general laws (*i.e.*, f_3 laws) which would connect the relative strengths of the customs of 'cooperation,' 'competition' and 'individualism' to such independent variables as economic and technological factors. But it did not succeed in finding any clear-cut relationships of this sort. The only important f_3 law that anthropologists have as yet uncovered seems to be merely the very general one of 'historical diffusion.'

independent variables for *C* and the sociological intervening variables above them. Also, I have drawn other double-headed dash arrows to suggest similar part-whole relationships between the psychological independent variables for each of the other individuals in the group and the sociological variables for the whole group. In short, the culture-pattern of the group appears to be a whole which is made up of the maintenance-schedules, the environmental object-patterns, the heredities, the previous trainings, the ages, etc. of all the various individuals within the group. But, if this be correct, then it appears that the psychologist's independent variables are not in any final sense independent and absolute. They are always immersed in a 'field' constituted by the 'culture-pattern' of the whole group. They cannot be manipulated wholly independently of this field.

Further, it is to be stressed that such a field—such a culture-pattern—is a relatively interconnected whole. Hence any experimental variation that the psychologist attempts to make in any one independent variable is almost surely bound to produce at the same time uncontrolled and unwanted changes in others of the independent variables also acting upon the given individual. Or, in short, psychology here comes up against two sorts of difficulty:

First, psychologists cannot study the f_2 function issuing from some one independent variable—for example, sex maintenance-schedule—for some one individual without danger of at the same time affecting, even though only in some slight degree, the sex *mores* of the whole group. And such a change in group *mores* will react back upon and affect all the other independent variables (*e.g.*, the environmental object-pattern of persons and things) also acting upon this same individual. Any one independent variable for any one individual is, in short, always part of a whole interconnected sociological field. And the laws found for any one such independent variable will always thus be colored by the latter's interconnections with all the other variables in this field.

And, secondly, even if psychology surmounts, to some extent, the above difficulty and tears the effects of the separate

independent variables apart, it will still remain true that the laws for individual behavior which psychology finally arrives at will be laws holding, so far as we can be certain, only within the given culture in which they have been found. Only by studying psychology within many cultures would it eventually be possible to arrive at a pure psychology—a psychology which one could feel certain would hold for all cultures.¹⁰

And thus we come to the first part of our original thesis—namely, that sociology is in some considerable measure ancillary to psychology.

Let us assume, however, that in spite of the above we do not give up but proceed to prosecute psychology as best we may. Let us look next at the 'intervening variables' as listed for psychology. I have designated them as 'demands, perceptions and hypotheses.'¹¹ These 'demands,' 'hypotheses' and 'perceptions' lead to *C*'s behavior of 'going to the wedding.' They would be: for example, the demand to win favor with the particular bridesmaid, the demand to make a good impression on her family, the demand for party-food—*i.e.*, sweets, liquor, etc.—, hypotheses as to the direction and distance of the church, hypotheses as to the probability of meeting the bridesmaid's family, perceptions of the concrete objects and persons involved in getting dressed, going to the church, and so on. These demands, hypotheses and perceptions¹² would combine together according to some f_s function (not as yet sufficiently known by psychologists) to produce the final behavior of 'going to the wedding.'

We note next, however, that such a larger set of demands, hypotheses and perceptions leading to the total behavior of 'going to the wedding' must be conceived as comprising a number of component sets of smaller demands, perceptions

¹⁰ And such a universal pure psychology might well turn out in the end to be restricted to but relatively simple and biologically conditioned facts. And, further, it may well be that this field of a pure biological (non-cultural) psychology is after all best investigated with animals (10).

¹¹ For the defense of such intervening variables for psychology, see Tolman (8, 9, 10).

¹² It is, of course, understood that these demands, hypotheses and perceptions are behavioristically, not introspectively, defined affairs (8).

and hypotheses. That is to say, just as the total behavior of 'going to the wedding' was conceived as containing within itself separable moments such as: 'getting dressed in such and such a fashion,' 'starting at the correct time,' 'stopping, bowing, talking,' etc., so now the general more inclusive set of demands, perceptions and hypotheses is to be conceived as containing within itself corresponding subordinate sets of smaller demands, perceptions and hypotheses.

The relationship of each of these minor sets to the more inclusive one, I have indicated by double-headed dash arrows (see legend). The relationship is again one of parts to whole. That is to say, in attempting to vary for experimental study any one such minor set of demands, perceptions and hypotheses occurring at some single moment such as *M*, we will thereby tend to change, at least in some degree, the larger more inclusive set. This latter will act as a 'field' within which the minor ones are enmeshed.

Or, in other words, we meet here again an analogous difficulty to that for the study of the behavior of single human individuals always immersed as we saw in larger cultural wholes. The intervening variables for the large behavior comprise and determine those for the smaller behaviors just as truly as conversely those for the smaller, included behaviors also constitute and determine those for the total large behavior.¹⁸ We cannot study the small behaviors without knowing also the laws of the larger including behaviors.

Physiology.—But turn now to the physiological level. Its relation to the psychological (behavioral) level above it is one of parallelism. This parallelism is to be conceived as present and is indicated by double-headed unbroken-line arrows for the independent and the intervening variables as well as for the dependent variables.

Consider, first, the independent variables. Corresponding and parallel to the independent variables as identified by the psychologist, the physiologist finds patterns of present and

¹⁸ Again it must be emphasized that this problem of the interrelationship between the superordinate and subordinate behaviors requires much more analysis and study than anyone has as yet given it.

past *stimuli* (exteroceptive, interoceptive and proprioceptive) plus such and such neural and biochemical make-ups. That is to say, stimuli and neural and biochemical make-ups are to be conceived as being the same events and processes which the psychologist calls maintenance-schedules, past and present environmental object-patterns, heredity, training, age, and the like. The difference is one of language and size of descriptive unit. But any 'operational' change made in the one set of entities will always be accompanied by some corresponding change in the other parallel set of entities.

Look, next, at the intervening variables. Here also we find a parallelism. For the physiologist the intervening variables are spatially, temporally, electrically, and perhaps also chemically, identifiable excitations within the nervous system.¹⁴ But these can be considered as parallel to the psychologist's 'intervening variables' of demands, perceptions and hypotheses. Further, just as the larger demands, perceptions and hypotheses, which are determinative of the total behavior of 'going to the wedding' are to be conceived as containing within themselves subordinate demands, perceptions and hypotheses occurring at moments *K*, *L*, *M*, etc., so here at the physiological level the larger set of neural excitations must be supposed to contain within themselves distinguishable narrower more momentary patterns of excitation.

What the above means is that before studying the laws governing any relatively small group of physiological activities (muscle contractions and gland secretions) such as those at moment *M*, we have to note, first, that such a minor group of physiological processes occurs only within some larger matrix of processes. Hence the laws which we, as physiologists, discover for the minor process probably only hold within the specific larger matrices in which they have been studied. And this limitation the physiologist (or at least the psychologist who poses as a physiologist) is apt to forget.

Furthermore, and this is the main practical burden of the argument, it seems it is just here that the psychologist is

¹⁴ Here again, as was the case with sociology, I feel myself treading upon relatively uncertain ground.

probably at present in the more strategic position. For the larger, controlling set of circumambient events (intervening variables) can today, it would seem, often be better identified and controlled in the psychologist's terms of demands, perceptions and hypotheses than in the parallel physiological ones of widespread (and not easily get-at-able) neural patterns of excitation. The physiologist who seeks to predict and control some narrow, neural phenomenon such as that leading to the specific muscle contractions and gland secretions at moment *M* can, I believe, do it today only by defining the circumambient and controlling matrix *psychologically rather than physiologically*.

And so we come to the second half of our original thesis—namely, that (at any rate, today, *in the present stage of the two sciences*) psychology tends to be in large part ancillary to physiology.¹⁵

In conclusion, let me briefly recapitulate:

(1) *Sociology versus psychology*.—It has appeared that we cannot (at least in the case of society-forming animals such as human beings) study psychology—*i.e.*, the *behavior* of individuals—save within larger sociological wholes. The intervening factors determining the *group conduct* act as a 'field' ¹⁶ determinative of the independent variables acting upon the individuals within the group. And this makes for two kinds of difficulty or limitation for psychology:

(a) The field presented by the social group tends to cause interfunctional relations between the psychologist's independent variables.

(b) Even if it prove in some measure possible to get around this first difficulty, there is still a second difficulty in that (having studied the f_2 and f_3 functions within merely one social group) we shall still have to study them over again within other different types of social group. For in such

¹⁵ This of course *does not mean* that, therefore, the physiologist and the physiological psychologist should now desist from continuing a very rigorous pursuance of the physiological facts *per se*. It may be that before long this present practical situation as between psychology and physiology may become reversed.

¹⁶ For the general importance of the methodological concept of the field, see Lewin (3) and Brown (2).

other types of group the independent variables of 'maintenance-schedule,' 'environmental pattern,' etc. may take on quite other and wholly new values, the effects of which cannot be extrapolated from the findings for the first type of group.

In a word, we are forced to conclude that sociology is to a surprising degree ancillary to psychology.

(2) *Psychology versus physiology*.—Psychology and physiology are parallel sciences—(i.e., a behavior-physiological parallelism, not a psycho-physical parallelism). From a purely theoretical point of view neither, therefore, would seem ancillary to the other. We note further, however, that for both sciences there appeared to be the difficulty that any relatively limited behavior (or correlated set of physiological activities) is practically always immersed in a 'field' constituted by a larger behavior (or larger set of correlated physiological activities). And this presents a difficulty analogous to that as between psychology and sociology. For it appears that to study any small behavior we have to know the larger behavior that this smaller one is immersed in; and, similarly, to study any smaller group of physiological activities we have to know the larger surrounding group of physiological activities.

And it also appears, further, that psychology is, today at any rate, here in perhaps the better position. For today it seems easier and more feasible to identify and control the larger circumambient processes in psychological rather than in physiological terms.

Hence we must conclude that (*practically speaking*) psychology today is also in some considerable degree ancillary to physiology.

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TASTE FOR AFFECT-GAIN

BY HELGE LUNDHOLM

Duke University

La vie aux enfers est extrêmement diminuée; on n'y sent ni plaisir ni peine; on est comme si l'on n'était pas (Anatole France, *L'île des Pingouins*, Livre III, Ch. VI: Marbode).

During prolonged endeavor to organize systematically the topic matter of two great branches of psychological science, the one of abnormal psychology and the one of the psychology of æsthetics, it has become increasingly apparent to me that certain pathological or near-pathological mental reactions, as well as certain reactions embodied in the complex process of æsthetic enjoyment, can be best explained as the expressions of an acquired desire for increased intensity of affect, as it were, for its own sake; or, at least, independently of any obvious purposive motivation.

The desire to experience affect as an autotelic or, at least *quasi-autotelic* end, I have accustomed myself to speak of as a *desire for affect-gain*. The term affect-gain was suggested by a similar expression used in the English translation of Ernst Kretschmer's monograph 'Hysteria.' Here, Kretschmer speaks of 'affective-gain' in one single connection, in one single passage which I quote in part.¹

A good example of the *mechanism of affective-gain*, as this typical hysterical process will be briefly called, is given by the following picture. . . . An inmate of a neurological station has an attack and gets into a loud dispute. If the physician will appear on the scene about half a minute later, he will already find the little space around the patient filled with closely packed groups of fellow-patients. . . . They are the same persons who hardly an hour before had complained of anxiety and sensitiveness to noise, and who now . . . forthwith go to the noisiest place. What are

¹ E. Kretschmer, *Hysteria, Nerv. & Ment. Disease Monog.*, 1926, 44, 61.

they all doing there? . . . *They are loading up with the emotional fuel which must continue to keep their reflex machine going.*

The desire for affect-gain described by Kretschmer is superimposed upon a specific neurotic condition. It would be futile to discuss it until this desire has been considered in its wider ramifications.

I believe that the first step in a general survey of the nature and origin of desire for affect-gain would be to bring it in connection with what is commonly known as *tastes for activities*. Though the role of tastes in the life of the civilized human adult is considerable, few psychologists have given attention to them. William McDougall, for one, has attempted a comprehensive treatment of the psychology of tastes. I shall briefly quote a few essential parts of his exposition and then I shall try to show that the desire for affect-gain presents unquestionable kinship with the *tastes proper* as described by him, although it also, in certain respects, differs from these.

McDougall in his most recent treatment of the tastes, in 'The energies of men,' writes:²

. . . we acquire likes and dislikes for particular kinds of activity, and these are properly called *tastes*. . . .

. . . sentiments are likings and dislikings for objects.

. . . While our sentiments determine the major goals towards which we strive, our tastes determine our choice of means . . . in pursuing these goals.

. . . Take the case of a youth who, on entering college, has a strong desire to distinguish himself. The desire springs from . . . his sentiment of self-regard. He . . . is ready to try . . . any one of the many possible lines of activity through which his goal may be attained. . . . He tries several of these avenues, and finds himself efficient . . . in one of them, say debating. He then concentrates his energies along this line of activity; and for two good reasons . . . [1] he sees that this line of activity offers the best prospect of attainment of his goal, namely, distinction . . . [2] in so far as he finds himself an efficient

² W. McDougall, *The energies of men*, New York: Scribners, 1933, pp. 239-241.

. . . debater, he will enjoy debating, experiencing frequently the satisfaction of successful activity. Thus, in accordance with the fundamental law of feeling (that we tend to persist in and renew the forms of activity that are pleasant because successful) he acquires a taste for debating. When this taste has been acquired, his motive for debating is still his desire for distinction . . . but the choice of this means (debating) towards his goal is confirmed by his acquired taste.

Suppose also that on trying several lines of athletic activity, he finds himself making good progress towards proficiency in one game, say tennis, while relatively . . . unpromising in others. He concentrates on tennis and, in so far as he continues to be successful in this game, he enjoys it and acquires a taste for it. . . .

Sentiments, then, . . . supply the motives of all sustained activities. . . . Tastes . . . are not in themselves sources of desires; they determine, not the goals, but only our choice of means. If a taste is to be indulged, there must be some motive springing from some source other than the taste itself. . . .

The tastes here described differ in important respects. The first taste (for debating) calls for the sagacious cognitive activity upon which swiftness of repartee is contingent, the second (for tennis) calls for motor skill combined with keenness of perception and of spatial judgment. Generally speaking, taste proper might develop whether for cognitive activity, for motor activity, or for activity involving both cognitive discernment and motor skill.

McDougall's statement regarding tastes might be summarized in two brief propositions:

I. Tastes develop for activities which have repeatedly served as successful means for goal-attainment.

II. As tastes are not springs of energy, indulgence of tastes is always energized by some sentiment.

On first view, both these propositions would seem to make unpropitious any attempt to interpret the desire for affect-gain as a variety of taste proper. For, though affective experience qualifies goal-seeking activity—indicating the na-

ture of needs and the progress or regress in the pursuit of goals—such experience can hardly be considered a means to an end in the same sense as effective planning through cognitive process (problem solving), or successful advance toward a desired goal through motor effort. With this consideration in view, how could a desire for experiencing affect develop in accord with the principle determining the development of tastes proper? Another difficulty in interpreting this desire as a variety of taste proper arises from the fact that, whereas tastes proper develop for activities which have successfully led to goal-attainment and which consequently are associated with the pleasure of satisfaction, the desire for affect-gain is not conspicuously a desire to experience pleasurable, but unpleasurable affect as well, such as grief, distress, fear, etc.

In summary, the seeming obstacles to interpreting desire for affect-gain as a variety of taste proper are two:

- I. The apparent difficulty in finding a sentiment the satisfaction of which would be contingent upon affective experience *per se*.
- II. The peculiar fact that desire for affect-gain, in so marked degree, embodies a desire for unpleasurable affective experience.

The observation that many people—perhaps particularly those of slightly neurotic disposition—seem to seek pain, physical or mental, and to secure satisfaction from painful experience, has been abundantly commented upon by psychologists and physiologists of the latter part of the nineteenth century. Many explanations have been offered; few have been even remotely convincing. Unable to review this voluminous literature, I shall merely take up two suggestions that seem quite pregnant. Both represent distinctly psychological attempts to account for the satisfaction derivable from pain.³

The first of Hirn's suggestions is to the effect that any intensive affective experience—even a painful one—is always accompanied by an enhanced awareness of vital existence. Such awareness, he thinks, is bound to be pleasurable. The

³ They are advanced by Yriö Hirn in his 'The Origins of Art.'

pleasurableness of enhanced awareness of vital existence, he intimates, might account for certain people's desire for pain.

Hirn's second suggestion is to the effect that any painful experience might be made a source of enjoyment by sentimental reflection; for instance, reflection upon the undeservedness of one's misery in view of one's great virtue, or any other reflection colored with self-pity.⁴

The validity of the second suggestion can hardly be questioned. Any psycho-pathologist has seen many instances of it; so has any student of literature. In an article of 1926, I called attention to patients in a non-retarded depression securing gratification of a morbid pride by proclaiming themselves at the very depth of misery.⁵ The reader might recall a scene from Shakespeare's 'King Richard III,' in which three royal mothers—all bereft of dear relatives through the villainy of the Duke of Gloucester—actually compete with one another in grief, each one insisting emphatically upon being more miserable than the rest; each one evidently finding some satisfaction in having been more cruelly treated than the others.⁶

If we examine Hirn's two accounts of the pleasure of pain—hereafter we shall call them his *first* and *second principle* respectively—we find that they have one common feature which is of significance for the interpretation of desire for affect-gain as a variety of taste. They both imply that painful affect might serve the satisfaction of an end, might, in other words, be a means to an end. What is this end? In the first case it is the pleasurable awareness of enhanced vital existence; in the second case it is the satisfaction effected by sentimental reflection colored by self-pity. None of these ends are purposes in a technical psychological sense. They can, however, by translation or re-interpretation, be fitted into a technical psychological system. As this is easier to accomplish with the second than with the first principle, I shall begin by examining the second principle.

⁴ Y. Hirn, *The origins of art*, London: Macmillan, 1900, pp. 60-64, p. 51.

⁵ H. Lundholm, *Constitutional psychological factors in functional psychoses*; I: Manic-depressive insanity, *J. Nerv. & Ment. Dis.*, 1926, 64.

⁶ W. Shakespeare, *The tragedy of King Richard III*, Act IV, sc. 4.

Is it not true that self-pity embodies a subjective elevation of self-esteem, a gratifying flattery of one's own self-regard? The very essence of sentimental reflection colored by self-pity would seem to be the elaboration of one's own undeservedness of misfortunes encountered. Undeservedness of misery, in turn, would seem to be contingent upon a comparatively high estimation of one's own merit or virtue. Suffering might thus, through sentimental reflection, become an indirect means of gratifying self-regard; and some individuals might seek pain in order to secure such gratification.

Hirn's first principle reads: intensive affective experience—including painful experience—always enhances one's awareness of vital existence; certain people might seek pain in order to enjoy the pleasure of this awareness. I suggest that the pleasurable awareness of vitality, also in such cases, is a pleasure arising from gratification of self-regard. Surely, it cannot be associated with the satisfaction of any one subordinate sentiment. It is contingent, rather, upon a subjective elevation of the total person. If, like McDougall, we consider the total person focused in a dominating self-regarding sentiment, the validity of my suggestion is strengthened. If this interpretation, which by no means gainsays introspective evidence, is accepted, we find a communal characteristic in Hirn's two principles, namely, that both the methods of deriving pleasure from pain, implied in these, succeed in so far as they are instruments of gratifying the self-regarding sentiment.

It is evident that recourse to painful affect-gain as a means of elevation is a weak human reaction bordering, in many instances, on the pathological. It is current among people who lack constructive purpose in life, or in people who lack courage to assert themselves by overt useful activity. In other words, we find it in men and women who would otherwise suffer from boredom or from inferiority feeling.

We have thus brought the desire for affect-gain into fairly close proximity to tastes proper. We have found that the experience of affect in itself might serve as a means of gratifying self-regard. As tastes in general develop for activities

which serve successfully as means to ends, there is no logical reason why affect, which repeatedly and successfully has served the elevation of self-regard, should not become the object of taste.

However, a minor difficulty remains in giving desire for affect-gain full rank among the tastes, a difficulty arising largely from the meaning common usage has attached to the word, activity. While the word, activity, is commonly applied to thinking and to overt behavior, it is not as commonly applied to the experiencing of affect. This is a failure accounted for by the fact that common language oftentimes does not lend itself to adequate scientific description. From the viewpoint of technical psychology, *experiencing affect*—as well as *thinking* and *conducting*—is activity.⁷

Tastes proper all concern successful cognitive processes or successful overt conduct—whether in separation or in combination. If we acknowledge that the third process characteristic of conation, the affective process—like thinking and moving—is activity, then there is no logical reason why we should not class as a type of taste the acquired desire to exercise the process (or activity) of experiencing and displaying emotion, whether simple emotion or emotion complicated by feeling.

It might still be objected that affective process is not open to voluntary control, like thinking and moving. This statement is only partly correct. It is true that affective process is not as directly controllable by will as, for instance, the motion of a limb. Yet affective process may be intentionally instigated in indirect ways, namely by spontaneous imagina-

⁷ In a recent work, I have pointed out that any pursuit of a goal incorporates three types of process (or activity), each of which prevails over the others upon adaptive need. These processes I have called the *meaning-process*, the *emoving-process*, and the *moving-process*. The meaning-process is the one of cognizing the goal, and includes the planning of goal-realization and the problem-solving upon which success is contingent. The emoving-process is the experiencing of emotion and indicates the nature of a need; this process, as a rule, becomes complicated by complex feeling (hope, despondency, regret, etc.) which qualifies progress or frustration during pursuit of a goal. The moving-process is the overt advance towards a goal. These three types of process are characteristic of any mode of conation, simple or complex. They are equally significant whether a man is prompted to endeavor by a simple instinct or by a complex sentiment such as the sentiment of self-regard, such as the sentiment of self-regard (H. Lundholm, *Conation and our conscious life*, Durham: Duke University Press, 1934, pp. 43-48).

tion of exciting situations, or by spontaneous search for them. This matter will be discussed later.

There remains a difference between the tastes proper and the taste for affect-gain. The former arise from successful activities prompted by a variety of sentiments, while the latter, if my interpretation is correct, is always associated with the self-regarding sentiment. This difference, I believe, is only superficial. I suggest that—no matter what sentiment prompts the activity for which a taste proper develops—the acquisition of proficiency in any performance will always gratify the self-regard of the performer. If this is true, the indulgence of a taste is bound to be energized not merely by the former sentiment but by the total person as reflected in his self-regard. May it not even be true that any pleasure in success, suffered by a human adult in whom impulsive life is integrated under dominating self-regard, must involve pleasure arising from the gratification of that sentiment? If my argument is valid, self-regard energizes not only the taste for affect-gain but also, in a measure, all the tastes proper.

As a rule, it is far more difficult to trace the origin of a person's taste for affect-gain than to trace the origin of a taste proper. His discovery that affect—even painful affect—may yield satisfaction is contingent upon a variety of activities. For each individual the discovery has its own history. The precipitating events leading to the discovery can rarely be assembled around the function of any special sentiment, as in the case of the tastes proper; it is only after the discovery is made and the taste formed that we can definitely associate the latter with the self-regarding sentiment.

In discussing the taste for affect-gain, I have centered my attention upon taste for unpleasurable affect. This I have done for two reasons: (1) intentional search for pain constitutes the most conspicuous form of taste for affect-gain, (2) this variety of taste for affect-gain constitutes the most difficult problem of the topic under survey. The problem of taste for pain once solved, the taste for pleasurable affect-gain requires no explanation of its own. It is easier to understand, being evidently accounted for by Hirn's first principle.

Hirn's second principle is obviously of no significance in the formation of taste for pleasurable effect.

In what manners can a person voluntarily indulge his taste for affect-gain? He can do it by exposing himself to exciting situations. This, in turn, he can achieve in two ways: (1) on the level of reality, and (2) on the level of unreality. In other words, he can seek exciting situations in real life, or he can seek such situations in imagination. Both methods are used, sometimes in separation, sometimes in combination. The sentimental reflection, constituting Hirn's second principle, practically always involves the accentuation of affect by continual imagination of painful situations.

When a taste for affect-gain is indulged on the level of reality, the exciting situation sought is frequently one in which affect is sympathetically induced by the observation of affective conduct (including vocal utterance, gesture, and mimicry) on the part of others. (Exceptions to this rule occur as will be seen later.) This has an interesting explanation.

By definition, a pure taste for affect-gain does not incorporate liking for the cognitive activity and the overt motor activity which, in normal life, serve the satisfaction of needs the natures of which are often-times indicated by the affect (as in the case of emotions). Consequently, whenever a person—for the sake of indulging his taste—seeks an exciting situation, he does not seek one in which his welfare requires adaptive cognitive activity or adaptive motor activity. He seeks, rather, a situation which offers him safety at the same time that it strongly stirs his affects. These criteria are well satisfied by situations in which overt emotional conduct on the part of others can be observed and permitted to excite affect sympathetically. As long as the observer himself is in safety, his sympathetically excited conation needs not express itself otherwise than in the experiencing of affect, all his energies being, so to speak, drained into affective process.

The restriction in the choice of exciting situations on the level of reality, does not apply to the level of unreality. In indulging his taste for affect-gain on the latter level, a person

has unlimited liberty. There is no imaginary situation—even of the most severe personal jeopardy—that might not be utilized in the pursuit of affect-gain.

In opening my discussion, I called attention to the fact that certain pathological reactions, on the one hand, and certain reactions embodied in æsthetic enjoyment, on the other hand, can be best understood as the expressions of an acquired desire to experience affective excitation, as it were, for its own sake, in other words, as the expression of what we might now call a taste for affect-gain. This proposition indicates that desire, or taste, for affect-gain might be disclosed upon very different levels of dignity. In fact, if we admit the aptness of applying to human activities a scale of dignity, such a scale can be applied to the large variety of manifestations of taste for affect-gain. Between the extreme ends of this scale falls a wide array of expressions of the taste, some pathological or near-pathological, others normal. Though it is difficult to place each type upon a precise point of the scale, and though individual opinion might differ as to the relative placing, I shall venture briefly to describe—in an ascendant series—a number of manifestations of taste for affect-gain. Before I proceed further, however, a general comment on the pathological forms of this taste is required.

Whenever desire for affect-gain appears as a symptom in a curable mental disease, it may not be fair to speak of a taste for affect-gain. For, in such instances, the desire is likely to vanish with recovery. The desire for affect-gain, then, characteristic of some patients suffering from agitated depression, should not be called a taste. The same would be the case with the desire for affect-gain described by Kretschmer and quoted in the beginning of this article. In this instance, the desire is contingent upon a specific hysterical condition. Recovery might here, as in the case of agitated depression, completely remove the desire.

The situation is quite different in cases of psychopathic inferiority in which a desire for affect-gain tends to develop into a chronic condition. Here, it is not contingent upon any

curable mental disease, but upon an inborn defect for which there is no remedy. Take, for instance, the type of psychopath known as *the wanderer*. He is described as a restless person who cannot remain in the same abode without boredom. As a consequence, he tramps around from place to place, continually and aimlessly, never being able to apply himself to any useful constructive enterprise. He seeks salvation from boredom in the thrill of travel. The thrill offers him the elevation of whatever he may possess of self-esteem (Hirn's first principle). Other psychopathic forms of thrill-seeking are probably of similar psychological main-spring. In instances of this kind I would hesitate less than in the acute cases to speak of taste for affect-gain.

In the survey of different varieties of taste for affect-gain I shall use two expressions which I wish to define before applying them. They are: *sympathetically excited affect* and *directly excited affect*. The former refers to affect evoked through passive sympathy, the latter to affect excited independently of passive sympathy.

Lowest on the scale of dignity of all tastes for affect-gain I would place the desire to watch cruelty exerted upon the defenseless, be he man or animal. Affect-gain in observing cruelty is secured through sympathetic excitation of fear and distress and, in rare cases—paradoxical as it may seem—through directly excited pity. The Romans of the period of decline excelled in this taste when they attended performances in which defenseless men and women were tortured and killed in the arena. Nearest, in modern times, to the Roman arena-performances come, no doubt, the Spanish bullfights.

Akin to, though higher than the taste for cruel performances, is the taste for attending fair, but dangerous, fights and athletic games; also risky circus-acts, etc. Affect-gain is here secured through anxiety of expectation, directly excited admiration, and, in case of injury to performers, through sympathetically excited distress and, sometimes, through directly excited pity. All these affects are sympathetically reinforced by the affective gestures and exclamations on the part of other spectators. Even athletic games which are

fair and perfectly safe serve people as instruments of indulging taste for affect-gain. There are persons who enjoy attending games although they do not know the rules. They are incapable of directly excited admiration and their affect-gain is entirely a matter of sympathetic contagion. They will frequently tell you that they go to the game in order to hear the people cheer. The affect-gain from watching athletic games is intensified in people who bet upon a party or an individual. Betting accentuates the anxiety with which the game or race is attended. All gambling secures part of its fascination from the fact that it effects affect-gain. Affect-gain may not be to everybody the most important factor in the enjoyment of games. However, I believe that comparatively few enthusiastic attendants of games fail to derive some enjoyment from affect-gain.

Next to cruel taste, at the lower end of the scale of dignity, I would place the taste for affect-gain commonly called scandal-hunger. The scandal-hungry enjoys his own moral indignation. He is frequently the carrier of gossip and, in gossiping, he is likely to magnify scandalous rumor. Gossiping, whenever a responsive audience is found, re-enforces his moral indignation through retroactive sympathetic contagion from the indignant group. This taste is often found in spinsters without occupation whose maintenance depends upon charity or upon the kindness of relatives. Affect-gain, by elevating their self-esteem, saves these unfortunate mortals from ennui or inferiority-feeling.

Scandal-hunger is a variety of a wider type of taste to which belongs also the cruel taste. Most broadly defined this taste might be characterized as a general taste for observing sensational events. Another variety of this taste is to be found in people who swarm to places where an accident is indicated in order to enjoy sympathetic distress, or who board their vehicle in pursuit of the fire engine in order to take advantage of the opportunity to enjoy anxiety and possibly sympathetic distress and fear. These individuals like to read in the daily papers and elsewhere about train- and ship-wrecks and about the cruelties of warfare. The

taste might also express itself in indulgence in cheap horror-fiction; in other instances, in frequent visits to the horror-chambers or anatomical sections of wax-museums, even in a liking to watch operations, deliveries, and executions whenever an opportunity is offered.

A taste for affect-gain different from the ones described, though not much higher in the scale of dignity, is the taste which expresses itself in continual complaint about misfortune and misery. It is essentially accounted for by Hirn's second principle. In other words, it is a taste for securing self-esteem through self-pity sustained by sentimental reflection. There are three factors involved in the indulgence of this taste: (1) sentimental reflection which fosters self-pity, (2) re-enforcement of self-pity through retroactive contagion from a sympathizing group, or, in case the group is cold and unsympathetic, enhancement of self-pity through newly stirred sentimental reflection, and (3) direct elevation of self-esteem through attention paid by sympathizers, an attention that otherwise the 'sufferer' would fail to attract. It is quite possible that the development of this taste is contingent upon a certain weakness of urge, the weakness that produces a dependent type of adult—in men, the *sissy-type*; in women, the *clinging-vine* type. In extreme instances of chronic complaining, where the taste becomes near-pathological, this proposition is even very plausible. Though milder in its expression, this taste has a certain similarity with the tendency to complain shown by many patients suffering from a non-retarded depression. The taste is frequently found in women bereft of husband and children, or in childless widows; in other words, in people who have lost an object of their dominating sentiment and have failed to find new constructive purposes in life.

The widowed royal mothers in 'Richard III,' who emphatically compete with one another in grief, offer a good illustration of the taste we have in view. Mrs. Gummidge, in Dickens' 'David Copperfield' is another instance of the same taste. Mrs. Gummidge is a childless widow. She has felt the pangs of deprivation and of loss of purpose in life.

Being without means, she has had to accept the continual hospitality of Mr. Peggoty. She feels the inferiority of her position and as a compensation she develops a taste for complaining. Her complaint leads to attention being paid her, her self-pity is thus enhanced, her self-esteem elevated. The remarkable feature with Mrs. Gummidge is that, when circumstances force upon her responsibility and the necessity of constructive purposive action, her taste for affect-gain through self-pity vanishes, and she becomes the capable head of Mr. Peggoty's household.

No matter which one of the tastes for affect-gain so far discussed one considers the highest, between it and the most dignified variety of the taste there is a large gap.

While taste for cruelty is the basest of all desires for affect-gain, the most dignified expression of such desire is to be found in the taste for æsthetic enjoyment.

The psychology of æsthetic enjoyment is a large and very difficult topic, the full treatment of which would require a volume of its own. Here, I can only briefly indicate its nature. In so far as great emotional art and sentimental art have an affective appeal, and in so far as experienced affect is a component in æsthetic enjoyment, affect-gain is a factor in appreciation, and love of art a variety of taste for affect-gain. But, no true æsthetic enjoyment is merely the enjoyment of affective process. In all true art-appreciation there enters a cognitive activity, the apprehension of the abstract or purely formal relations inherent in the art-object. Success in this cognitive activity brings a satisfaction of its own which is part of enjoyment. A proper balance between the affective and the cognitive processes conditions what is called *æsthetic* or *psychic distance* in art. Undue prevalence of the former is likely to break the distance and has to be counteracted by an adequate measure of the latter. Often-times this is spoken of as the necessity of balance between emotional and formal appeal. The range of affects which might enter into æsthetic enjoyment has no determinable limit. Perhaps every affect a human being is able to suffer might be a constituent in the experience of beauty.

My surmise is that all affect that is part of art-appreciation—except the pure pleasure of successful apprehension of abstract relations—is sympathetically excited. In other words, an art-object induces affect in so far as (explicitly or implicitly) it stands for, or symbolizes an affective gesture, the latter term taken in a very broad sense. The defense of this theory is such an elaborate task that I cannot undertake even to indicate it here.

In view of the fact that the indulgence of a taste for affect-gain requires a certain measure of overt activity, such as throwing dice (in the case of gambling), open complaint, singing and praying at funeral or revival meetings, etc., some one might claim that the taste for affect-gain is merely a taste for these activities and thus of the nature of tastes proper. This, to my mind, would be an absurd misplacement of emphasis. I would insist that taste for affect-gain is a species of its own to be recognized along with the tastes proper.

As a whole, the scale of dignity of tastes for affect-gain occupies, in a way, a lower plane than the corresponding scale of tastes proper. This should not be understood to exclude the overlapping of the two scales. For instance, I would unhesitatingly place the æsthetic taste above a taste for fox-trotting. On the other hand, I would never place even the most refined æsthetic taste on the same high level of dignity as a taste proper for just acts, sprung from a genuine sentiment of love of justice.

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HISTORICAL NOTES ON CHILD ANIMISM

BY WAYNE DENNIS

Clark University

During the past twenty years child psychology has been greatly concerned with tests and measures. Although the products of child thought have been the subject of much study, they have been examined only for the purpose of scoring them as correct or incorrect from the adult point of view and converting them into increments of mental growth. For this reason Piaget's studies of the content of child thought (13, 14, 15) give the impression of being entirely novel. Yet the consideration arises that if thought tendencies can be discovered simply by listening to and observing the child, as Piaget claims, then the peculiarities of childish thought must have been discovered long ago. The answer is that they were discovered long ago. The recent rise of laboratory psychology has been accompanied by a loss of interest, and often by a loss of information, in respect to certain older problems. Piaget's work is a departure with respect to our immediate period in psychology, but observations similar to his have been recurrent over a period of two centuries.

There can be no doubt that through the work of Piaget the thought tendencies of the child have been described more fully than by his predecessors. In bringing to attention the older references to child animism, I have no wish to detract from the credit of Piaget. This author's views should gain even fuller acceptance when it is more widely appreciated that some of his basic findings have been anticipated by outstanding figures in intellectual history.

The present discussion will be limited to what Piaget has called 'animism.' By animism Piaget refers to the attribution of life to objects which educated adults classify as inanimate. It is, perhaps, unfortunate that Piaget decided

to use the term 'animism' in this connection, as Tylor (23) has made the term famous in a very different usage.

It should be emphasized that this discussion does not attempt to assemble evidence for or against an animistic tendency in children. The sole object of the writer is to sketch the treatment of the topic prior to the twentieth century.

To make clear the intended limitations of the paper, it should be further called to the reader's attention that these historical notes are concerned with animism only as it is displayed by children. The animistic tendency in a wider sense has a vast literature which it is beyond my intention to survey. That discussion of this topic is old is shown by the fact that in Grecian civilization the animistic tendency was recognized as early as 540 B.C., when Zenophanes (12, p. 15) is believed to have written that if horses and oxen thought in the human manner they would imagine gods in the forms of horses and oxen. An early modern discussion of the animistic tendency is that contained in Hume's 'Natural History of Religion' (10, pp. 429-430). Hume's words in this connection were as follows: "There is a universal tendency among mankind to conceive all beings like themselves, and to transfer to every object those qualities with which they are familiarly acquainted, and of which they are intimately conscious. We find human faces in the moon, armies in the clouds, and by a natural propensity ascribe malice or good-will to everything that hurts or pleases us. . . . Nay, philosophers cannot entirely exempt themselves from this natural frailty, but have oft ascribed to inanimate matter the horror of a vacuum, sympathies, antipathies, and other affections of human nature."

The present paper intentionally excludes *adult* animism which is so well described by Hume. Its well-attested nature, however, should lead us to beware of a view which implies that animism is a trait present only in childhood and not found in adults. Piaget often seems to make this assumption.

To the writer's knowledge, the first explicit mention of the fact that an anthropomorphic or animistic tendency is to be

observed in *children* appeared relatively late in the history of human thought, although not so late as is supposed by those who would place all of the major observations upon children within the past fifteen or twenty years.

Tiedemann, the first person to write down detailed observations on child development, wrote in 1787 (22, p. 229). He recorded several instances of child animism. Tiedemann said, regarding his son: "I held my watch to his ear; when he had listened for a while to its beat, he said that Fripon (a little dog in the house) was imprisoned therein." Another instance is as follows: "In the evening, when he no longer saw the sun in the sky, he said, 'The sun has gone to bed; tomorrow he will get up again, drink his tea and eat his bread and butter.'" These observations are quite comparable to those of Piaget. The author stated that these and other ideas originated purely in his child's own deliberations; no one had told him that sort of thing.

Tiedemann not only presented evidence for the spontaneous tendency under discussion but proposed an explanation which I dare say is as good as we have heard since his time. While Tiedemann did not refer to other writers, it is likely that the theory was well-known and that he did not consider that he was proposing a new view. He held that the animistic tendency ". . . is certainly due to the fact that one always envisages an unknown thing through one that is known; . . . now there is nothing nearer and more familiar to us than ourselves, wherefore images of our own reactions, our own way of doing things, are constantly mingled with our ideas of external objects; therefore, we conceive all things as being like us, alive as ourselves, and acting by the same powers and motives as we do."

The same notions were expressed by Maine de Biran in his essay on 'The Influence of Habit on the Faculty of Thinking' (11, p. 227): "As the child, misled by some rough resemblances in form, clothes, etc., applies to the stranger the tender name of father, so the man—still a child—extends his *ego* over the whole of nature, attributes his will, his own activity, to all that sustains with him the general relation of

mobility, animates with his soul the stars, the clouds, the rivers, the plants, and peoples with genii, with active powers, the sky and the earth." Maine de Biran did not limit this extension of the ego to the adult period but held that the same principle of extension or generalization of habit exists from the cradle onward through life.

Froebel presented an excellent description of child nature in respect to animism, as he did in many other directions. He stated: "The child imparts to each thing the faculties of life, feeling, and speech. Of everything he imagines that it can hear. He imputes his own activity to all about him, to the pebble and chip of wood, to the plant, the flower and the animal" (7, p. 54).

A mentality similar to that just described played a basic part in Comte's theory of religion. Comte (4, p. 155) proposed that the earliest forms of religion were based upon ". . . that tendency of our nature by which Man conceives all external bodies as animated by a life analogous to his own." Comte claimed that this trend may be characteristic of the higher infra-human animals, as well as of the child and of primitive man. He said: "If, for instance, we exhibit a watch to a child or a savage, on the one hand, and a dog or a monkey, on the other, there will be no great difference in their way of regarding the new object, further than their form of expression:—each will suppose it a sort of animal . . ." (4, pp. 156-157). With this condition as the starting point, Comte saw man's ideas developing through a series of *stages*, and thus he was in this further respect a forerunner of Piaget. Comte believed that later thought stages do not completely wipe out the earlier one. Although the second and third stages may replace the first in most respects, Comte stated with regard to the animistic stage: "We may recognize some features of that state of mind in ourselves when we are betrayed into searching after the mode of production of phenomena of whose natural laws we are ignorant" (4, p. 157).

We come now to a man whose name is always associated with animism, but with a different kind of animism, as was indicated earlier in this paper. E. B. Tylor (23) is famous

not only for stressing the ubiquitous character of soul-concepts but also for his theory of the origin of these concepts from primitive man's grappling with the facts of trances, dreams, visions, and the facts of sleep, unconsciousness, and death. In fact, Tylor proved so well the importance of the concepts of souls and of spirits that he has been accused of denying, or overlooking, or ignoring the simpler anthropomorphic tendencies which would endow objects, plants, and animals with human characteristics without necessarily granting souls to them.

It is true that Tylor had no occasion to stress the importance of a simple anthropomorphizing tendency, for that was not his problem. On the other hand, it is untrue that he denied or ignored it; to the contrary, he declared for it in explicit terms. Tylor wrote as follows: "Let us put this doctrine of universal vitality to a test of direct evidence, lest readers new to the subject should suppose it a modern philosophical fiction. . . . Even in civilized countries, it makes its appearance as the child's early theory of the outer world, nor can we fail to see how this comes to pass. The first beings that children learn to understand something of are human beings, and especially their own selves; and the first explanation, as though chairs and sticks and wooden horses were actuated by the same sort of personal will as nurses and children and kittens. Thus, infants take their first step in mythology by contriving, like Cosette with her doll, '*se figurer que quelque chose est quelqu'un*'; and the way in which this childlike theory has to be unlearned in the course of education shows how primitive it is" (23, pp. 285-286).

Shortly after 1875 there came the beginnings of modern child psychology in the form of many biographical studies of infants. As might be expected, these detailed observations of children yielded further evidence of the animism which had been noticed by casual observers some centuries earlier. For instance, Taine (20, p. 258) noted, regarding his daughter, that "Animal or tree, she immediately treats it as a person and wants to know its thoughts and words; that is what she cares about; by a spontaneous induction she imagines it like

herself, like us; she humanizes it. This disposition is found among primitive peoples. . . ."

Preyer (16), who in most respects set the pace for later biographers, had rather little to say concerning thought tendencies in the child. He recorded without comment that many children regard a locomotive as tired when it stands still and as thirsty when its tank is filling. Nearly all of the other writers of the biographical school, of which there are more than fifty (5), have presented some records of this sort. It would be out of the question to cite further from such a long list of materials. Piaget, in 'The Child's Conception of the World,' makes use of the material only of Sully and Rasmussen from among the biographers.

It was not long after the beginning of the biographical movement in child psychology that a form of survey questionnaire was introduced. In America, the leader was G. Stanley Hall, and his publication, 'The Contents of Children's Minds' (8), undoubtedly will receive mention in all histories of child psychology. His method in this study, while not so refined, was not unlike Piaget's so-called clinical method.

In connection with Hall's famous article, it must be pointed out that a large part of the publication has nothing to do with the child's thought tendencies and this fact may possibly account for Piaget's failure to make any reference to a study famous in the American field. The major problem with which Hall was concerned was the percentage of children entering school who were familiar with certain common but ill-defined concepts. The section of his work which deals with this problem must be considered as analogous to mental tests rather than as analogous to Piaget's efforts to study the 'structure of the mind.' In contrast with the more or less practical tone of the first part of his publication, the later portion of Hall's work did attempt to delineate the main features of the child's thinking rather than to undertake a counting of concepts. In this later discussion Hall recognized the same difficulties in the execution of his task that are discussed by Piaget in the introductory chapters to 'The Child's Conception of the World.' Hall stated that if children are pressed to

answer questions somewhat beyond their ken they often reply confusedly and at random (compare with Piaget's 'answer at random'). Some are bolder and invent things on the spot (compare Piaget's 'romancing'). Others are quick to catch subtle suggestions from the form of the question, the voice and features of the questioner (compare 'suggested conviction'). But Hall insisted, as does Piaget, that in spite of these pit-falls "there are certain elements which every tactful and experienced friend of children learns to distinguish from each of these with considerable accuracy." Hall then proceeded to describe some of these elements, but he did so in specific rather than in general terms. He did not use abstract terms such as animism and artificialism, but he showed in detail that many children believe that the doll can feel cold, that it pains flowers to tear or burn them, and that the sky, the stars, the clouds, lightning and thunder, and other parts of the child's environment are conceived as man-like in their motive. Hall found that of 48 children questioned, twenty believed the sun, moon, or stars to be alive.

He further stated that ideas of this sort seem to be already on the ebb in children of the age of those whom he examined. Nevertheless, he believed that these concepts "represent a drift of consentient infantile philosophy about the universe not without systematic coherence. . . ."

The next child psychologist to be considered is Sully (19). Many writers have been moved to comment on the understanding with which Sully wrote his 'Studies in Childhood,' and Piaget has acknowledged his indebtedness to the English psychologist. Sully's book was essentially a collection of essays, not a system of child psychology, yet it is difficult to find a part of the 'Child's Conception of the World' which is not in some way anticipated by the earlier writer. In his chapter on 'The Products of Child Thought' Sully stated that there is no doubt that children, by reflecting on what they see or otherwise experience and by reflecting on what they are told by others, fashion their own ideas of natural phenomena; and further that, while these ideas are not a logical whole, certain general tendencies run throughout them.

Sully, in agreement with Hall, distinguished between the romancing and the serious moods in the child. The serious convictions of the child are what we are here to consider. In Sully's words, "Of these the most important is the impulse to think of what is far off, whether in space or time, and so unobservable, as like what is near and observed. Along with this tendency, or rather one particular development of it, there goes the disposition already illustrated, to vivify nature, to personify things and so to assimilate their behaviour to the child's own and to explain the origin of things by ideas of making and aiming at some purpose. . . . All apparently spontaneous or self-caused movements are . . . taken by children . . . to be the sign of life, the outcome of something analogous to their own impulses. . . . Next to movement, apparently spontaneous sound appears to be a common reason for attributing life to inanimate objects. . . . Children's ideas of natural objects are anthropomorphic, not merely as reflecting their own life, but as modelled after the analogy of the effects of their action" (19, Chap. 4). Sully gave a number of observations to illustrate these and similar generalizations.¹

Subsequent to Sully, the subject of child thought continued to interest child psychologists and additional observations were accumulated. Tanner (21), in a general treatise on the child, discussed animism and personification. Whiting (24) described a case of strong personification of numerals in a child. Herrick (9) reported a similar case and incidentally made observations upon imagery which seems to have been eidetic in its character. Brown's study of children's thoughts and reasonings (3) is referred to by Piaget. Barnes's (2, 448) investigation resulted in this very practical conclusion: "If young children are to be taught a theology, it must have an anthropomorphic and realistic form. We may teach that God is a spirit, but the child's mind at once invests him with

¹ In addition to an account of the animistic tendency, Sully also presented an extensive discussion of what he calls 'natural realism' and which is practically synonymous with Piaget's concept of child realism. He noted, for example, that for the young child the dream is in the bedroom, and that thoughts come out of the mouth; but to present this material is beyond the scope of the present task.

a human form and human attributes." Ament (1, pp. 190-195) presented an excellent account of animism. Slaughter's treatment (18) of child animism is good and contains supporting material. The date of her article is 1902, the date also of Ellis's article (6). Soon thereafter the testing movement began to develop. With absorption in this new practical domain, the interest in a descriptive account of the child lessened, and one almost fails to find references to thought tendencies in the rush to measure mental growth. There were, of course, some exceptions to the general trend. Rasmussen (17), for instance, was the author of an excellent discussion of thought tendencies; but the American textbooks of today characteristically make little or no mention of animism.

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A NOTE ON THE DIFFERENCE BETWEEN THE MORO REFLEX AND THE STARTLE PATTERN

BY WILLIAM A. HUNT AND CARNEY LANDIS

New York State Psychiatric Institute

In his recent article entitled 'The Organization of Behavior in the New Born Infant,' Karl C. Pratt (6) has identified the Moro reflex with the startle pattern by the phrase ". . . the 'startle,' which from Hunt and Landis' description is identifiable as the Moro response. . . ." The description on which he bases this identity mentions "a forward movement of the head, shutting of the eyes, a characteristic distortion of the features, a raising and drawing forward of the shoulders, an abduction, forward elevation, and inner rotation of the upper arms, bending of the elbows, pronation of the fore arms, clasping of the hands, contraction of the abdomen, forward movement of the trunk, bending of the legs at the hips and knees, and accompanying random foot movements" (3). A line drawing in the same reference illustrates the startle pattern clearly and shows that it differs from the standard Moro picture by the customary absence of extension movements. In the past, the term 'startle' has been used in a careless, omnibus fashion to include any response to a sudden, unexpected stimulus. This use of the term has led Peiper (5) to identify the Moro reflex with startle, and no doubt this confusion has helped Pratt to make the mistake of identifying the Moro and the startle pattern. It was to avoid this confusion that we used the phrase 'startle pattern' and took such pains to define it both verbally and pictorially. The startle pattern is primarily a flexion response, while the Moro reflex is primarily an extension response. In the Moro we find extension of the arms out at the side at right angles to the body, extension of the fingers, and frequently extension of the trunk and head. In the startle pattern, the arms are

never raised up and out in this extreme fashion, the fingers are flexed, there is a forward, flexion movement of the trunk, and the head moves forward also in flexion. These differences are quite noticeable, and in his original work Strauss speaks of the Moro reflex and the startle pattern as two separate responses (7). The points of similarity which do exist—and they are strongest in the movements of the lower extremities, which are flexed in both responses—are not sufficiently numerous to overcome the many points of difference. These differences could only be explained away by some doctrine which involved a progressive degeneration and decay of the original Moro reflex until it assumed the form of the startle pattern. Such changes are known to occur in the course of neurological development, but there is little reason for assuming their presence here. This possibility, however, has been considered in another of our publications (2). The objections to it were stated there, but at the time we did not feel we had sufficient evidence to eliminate it completely as a possibility. Further work has led us to reject it unconditionally and to conclude that the Moro reflex and the startle pattern are distinct and individual responses. In addition to stressing the flexion-extension difficulties mentioned above, we have noticed many cases where the same stimulus will call forth both responses. Ultra-rapid photography as a technique of observation makes it possible in these cases to separate clearly the immediate, rapid startle pattern from the subsequent, more leisurely Moro reflex (4). This would seem to be a conclusive argument against their identity. It therefore seems best to accept their difference. The Moro reflex is most noticeable in the early days of life. It disappears rapidly and is usually absent after the fourth month of life. The startle pattern is less regular during the first days. It becomes more regular as the child grows older, and perseveres throughout life. Whereas the Moro seems to be a phenomenon representative of cerebral immaturity, the startle pattern is present in normal maturity. We have prepared a more thorough discussion of their relationship in connection with a study of certain aspects of the Moro reflex (1).

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Wayne County Training School

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